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	PAGE
SOME ASPECTS OF PARANORMAL HEALING BY LOUIS ROSE, M.R.C.S., L.R.C.P.	105
REPORT ON THE OLIVER LODGE POSTHUMOUS TEST . . . . .	121
ESP TESTS WITH EROTIC SYMBOLS: CORREC- TIONS, AND INTERPRETATION OF THE RESULTS BY G. W. FISK AND D. J. WEST . . . . .	134
CORRESPONDENCE	
RANDOMNESS: THE BACKGROUND, AND SOME NEW INVESTIGATIONS (N. YOUNG AND J. FRASER NICOL); AN EXPERIMENT WITH RANDOM NUMBERS (B. BABINGTON SMITH, A. T. ORAM, AND CHRISTOPHER SCOTT); ESP EXPERIMENTS WITH MARIA (JOHN AND PATRICIA LANGDON-DAVIES); POLTERGEISTS: A PHYSICAL THEORY (HERE- WARD CARRINGTON & NANDOR FODOR, DENIS CHESTERS, AND G. W. LAMBERT); DR GELEY'S REPORTS ON THE MEDIUM EVA C. (THEODORE BESTERMAN) . . . . .	136
NEWS AND NOTES . . . . .	155

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SOME ASPECTS  
OF PARANORMAL HEALING<sup>1</sup>

BY LOUIS ROSE, M.R.C.S., L.R.C.P.

THIS is the record of an attempt to survey the nature and results of unorthodox ('paranormal') healing.

The attitude of early man towards the nature and treatment of disease was influenced by magic and superstition and later by formalized religion. The use of magical rites was followed by recourse to demons and gods and subsequently to an all-powerful god to whom prayers might be offered up for relief from disease (1 and 2) and relief was frequently experienced. In the sixteenth century academic medicine following in the Hippocratic path began to diverge from superstition and from religious influence, although sorcery continued to play a part in medical practice. Dawson (3) describes the activities of priests and physicians in Persia, China, India, and Japan, attenuated forms of which persist as in Vedic medicine today. Within the authority of the established Church healing was practised by the laying-on of hands, by prayer and by anointing for the first three hundred years A.D. (4) and these activities were frequently associated with magic and superstition. There has been a revival of the Church's interest in healing during recent years. Today in China and Africa the activities of the medicine man are thought to be on the increase even where the Christian faith is adhered to. Dean Inge says, 'It is quite unnecessary to go to Australia or Central Africa to find the savage: he is our next-door neighbour. The mentality of the stone age exists on our platforms and in our pulpits. There is no superstition too absurd to find credence in modern England—fetishes and tabus dominate London drawing-rooms' (5).

<sup>1</sup> This paper, the outcome of an enquiry undertaken at the suggestion of the Society's Organizing Secretary, was published in a slightly shortened form in the *British Medical Journal* for 4 December 1954. The present version is printed by courtesy of the Editor.

In many countries it is claimed that sufferers from all kinds of disability have been cured, or at least markedly relieved, by intervention of a 'healer' when orthodox medical treatment has failed. Healing shrines such as Lourdes are not considered in this report since religious faith is the major factor in that healing situation and no individual person is deemed to act as a healer endowed with specific powers. There are shrines of a similar kind elsewhere ; for example, in Persia. Organizations such as the City Temple (6) and the Guild of Health may combine faith and individual healing with orthodox medical treatment so that no clear-cut estimation can be made of the part played by either. As explained later, this problem also arises in absent healing. (Father Robertus of Rapperswill in Switzerland is reputed to be a healer of animals, obtaining results by oral communication.) During the past ten years Delawarr has elaborated a diagnostic 'camera' and a therapeutic radiation apparatus. These are claimed to provide diagnoses with the aid of a photographic plate which acquires a picture when a 'sensitive' operator makes suitable contact between a (blood) sample and the camera. This specimen of blood, submitted to the rays of the therapy apparatus operated by the 'sensitive', is said to produce improvement in the local or general health of the sufferer at whatever distance. Later photographs derived from the *same* blood specimens are claimed to reveal changes in the state of the subject. The evidence upon which these claims are based has not been made available. The 'science' of 'radiesthesia' was the subject of a special enquiry from whose report the following is extracted : ' . . . une expérimentation de "radiesthésie médicale", poursuivie selon les données de "radiesthésistes" et sous contrôle scientifique, a donné un résultat totalement négatif, les diagnostics exacts ayant été inférieurs en nombre à ce que le seul hasard aurait pu moyennement amener . . . ' (7).

Various forms of paranormal diagnosis are claimed by clairvoyants and mediums, and treatment may be applied in accordance with spiritualist principles (8). Among medically qualified healers Knowles (9) working in India has accumulated material in this field, and Dr Francesco Racanelli is working in Florence. Dr C. Woodard (10) cites no records suitable for critical survey.

Notoriously, throughout the ages, warts are likely to respond to any method other than the orthodox treatment *of the time*. Orthodox suggestion with or without hypnosis in the hospital or consulting room is less likely to result in the disappearance of a wart than, for example, the burying of half a potato in the light of a full moon. It may be of significance that many forms of treatment which are successful while the *rationale* is accepted by the therapist



begin to fail when new information casts doubt on this acceptance. The sword of logic pierces the shield of conviction and treatment technique is no longer successful—one of the main factors in the 'suggestion' situation having been eliminated. Too much, but still not enough, knowledge may thus be a handicap in therapeutics.

'Healing' for the present purpose is taken to mean the inducement of a state of comparative well-being by some form of personal contact, physical or mental. Harry Edwards (11) states, 'With contact healing, there is the tangible human link of the healing medium between the healing spirit guide and the patient'. The 'physical' approach may include 'manipulation' of a disabled part of the body and laying hands upon some affected organ or upon a part of the body near its site. ('Manipulation', in this context, is an assisted encouragement of the subject to move, or allow to be moved, an affected part of the body.) The 'mental' method, in general, involves some form of prayer for the well-being of the sufferer and a communication from healer to patient by mental application at a distance; in other words, a telepathic communication confirmed in writing. Quite frequently one finds the healer bringing his influence to bear while orthodox treatment continues—the former claiming the credit. Absent healing is 'a progressive process in which the first phase is a toning up of the general body condition', with concomitant 'increased vitality, brighter outlook and sounder sleep'. 'In the second phase, as the causes are removed, so the symptoms of pain and discomfort lessen and disappear. Healing is very often received during sleep'. The weekly letter from the healer to the patient is required 'to maintain the continuity of the healing' and 'every case is treated by the healer *and his co-operators*' (12).

These weekly reports which give the healer a picture upon which he can base his therapeutic activity must involve an enormous volume of correspondence and it is difficult to apprehend how any one individual could form even a fraction of the number of 'mental pictures' demanded of him for healing purposes. Thousands, to whom some 'message' must be passed, wait for a sign, some not even knowing that absent healing had been asked on their behalf. What does, in fact, take place?

Demonstrations of healing were attended by the author for the purpose of forming an opinion about the nature of this work and its results. The healer called for one or two people suffering from a specified malady, for example, 'arthritis', 'paralysis', and so forth. Patients suffering from 'incurable diseases' who had 'not been helped by doctors' were to be healed by 'spirit power' derived 'not from us'—'we are agents—human contacts'. He moved

arthritic joints, ran his finger up and down deformed spines and encouraged people to walk, stand up straight and so on. (When given the opportunity of approaching a patient suffering from scoliosis the circumstances did not permit of his examination ; I was merely asked to agree that there was 'spasticity' of the spinal muscles—a statement which I rejected publicly. On the realized agreeing, without checking the validity of the statement, I point of how difficult it is, in the face of authoritative suggestion, for the subject not to accept any proposition put by the 'healer'. I was not invited to approach another patient.)

Some patients seemed to receive immediate benefit in that they exhibited comparatively free, painless movement of joints which had apparently withstood routine medical treatment for years ; but one woman, who was able to leave the platform without the two sticks she used before being treated, was again leaning heavily on them in the street afterwards.

The phenomenon savoured strongly of suggestion but, in view of the vast following of healers, this investigation was embarked upon in a philosophical attitude, searching for truth without any attempt to refute, from the start, the claims. Does anything happen to the sufferer treated by a healer and if so, what and why? The questions involve a study of the disability treated ; does it improve or does the patient's attitude towards it change, bringing with it apparent improvement of the specific and general condition? If there is any measurable improvement either in the disability itself or the patient's attitude, is this amelioration linked with a specific psychological attribute essentially present in the sufferer or in the healer, or in both? If this were to be the case, what is the means of communication in healing? What is achieved by personal contact? How does absent healing operate ; is it some form of telepathy?

About this time it was reported in the press that Mr Harry Edwards had received his millionth letter during four and a half years of healing work at Shere. Analysis of this statement reveals that Mr Edwards and his staff must read two letters a minute over a reasonable working week, not counting time necessary for dealing with replies and instructions—and healing. However, progress was made in the investigation of individual cases reported in the press and elsewhere, including some submitted by Mr Edwards himself. Later, space was granted in the *British Medical Journal* to invite colleagues with knowledge of cases successfully treated by a healer, to supply, with the patients' permission, copies of medical records or other relevant information. Many non-medical people concerned with healing co-operated. It should be said here that



Mr Edwards has been absolutely ready at all times to reply to questions and to offer facilities for investigation with every evidence of good faith. More recently he has quite independently made efforts to expand the case-material by inviting 'healed' subjects to communicate directly.

In September 1951, at the Royal Festival Hall, another well-advertised demonstration was attended and treatment proceedings appeared to be more or less identical, with one exception. The healer at this demonstration seemed to be editing statements of patients for relay to the audience in such a way as to imply either greater or more prolonged disability before treatment, or better results following his treatment, than the actual words of the patients warranted. For example, he would ask, 'You couldn't do that before?' referring to the ability to lift an arm, and the patient would reply, 'Hardly ever, it was too painful,' following which the audience were told that the patient had not been able to do such and such before. It is, of course, difficult to substantiate this without a sound-recording, but another observer, Mrs K. M. Goldney, of the Society for Psychical Research, was in full agreement about this. It is possible that a similar process operated at the earlier demonstration but relative inexperience may have contributed to failure to observe this. It should be remembered that this was a 'demonstration' of healing and such 'manipulation' of statements might be thought necessary and desirable in the circumstances. A case treated that evening is discussed later.

In the summer of 1952 Mr Edwards's treatment centre in Surrey was visited. Here again Mr Edwards exhibited a preference for treating arthritis, spinal curvatures, spastic and other deformities. Usually he asked the patient for the medical diagnosis of the condition and he appeared to be somewhat guarded about the prognosis. He would run his hand lightly down a subject's back or other part of the body and stop, saying, 'This is where the trouble is.' It may be of some significance that to some patients he says, 'I *hope* you will be better,' and to others, 'You *are* better' or 'You will be better now.' This implies a clinical acumen enabling the healer to differentiate between cases in which strong suggestion is more likely to be of value than management and persuasion.

During the past three years many attempts with varying response have been made to gain information from interested observers or practitioners. The present investigation was discussed at a private meeting including three healers; one catapulted into theosophical matters and could not be confined to the detailed record of a case; the second came only on condition that he was not asked to contribute and the third did not talk at all. The co-operation of two

hospitals for 'Incurables' was requested ; one refused to consider the proposal and the Medical Superintendent of the other wrote that patients 'received and still do receive medical and surgical treatment likely to ameliorate their conditions. I feel, therefore, that psychic healing has no place in this institution'. Explanatory letters and franked envelopes were sent to patients claimed to have received benefit from healing ; these were accompanied by a consent form to facilitate the routine request for medical records. Some did not reply at all and many replied in terms which made further follow-up certain to be fruitless. Of those followed up a few were interviewed and their statements checked against medical records so that a reasonable estimate could be made of the results of healing. It is necessary to emphasize the stress which had been laid upon the so-called 'miraculous' cures of the patients investigated and the paucity of suitable case-material actually revealed ; a few short case-reports follow which illustrate the difficulties already mentioned.

This investigation covers over ninety such cases. Most of them were treated by one healer but all were claimed in the press or by the healer, or patient, to have derived considerable benefit after orthodox medical treatment had failed ; so that it is reasonable to assume for the present purpose that the results of other or less well-known healers will not differ greatly. Mr Edwards (14) claims to benefit 85 per cent of all cases brought to his notice—a number running into seven figures. Many doctors have failed to reply to specific enquiry, and it may be that any kind of enquiry into 'paranormal' phenomena (the existence of which is now recognized by the Churches, which 'can no longer ignore the body of evidence') is thought to savour of unorthodoxy and is to be avoided. This may account for the absence hitherto of any medically inspired research in this field.

#### CLASSIFICATION OF FINDINGS

(1) In 58 cases it was not possible to obtain medical or other records so that the claims remain unconfirmed.

(2) In 22 cases, records were so much at variance with the claims that it was considered useless to continue the investigation further.

(3) In 2 cases the evidence in the medical records suggests that the healer may have contributed to amelioration of an organic condition.

(4) In 1 case demonstrable organic disability was relieved or cured after intervention of the healer.

(5) 3 cases improved but relapsed.

(6) 4 cases showed a satisfactory degree of improvement in



function although re-examination and comparison of medical records revealed no change in the organic state.

(7) In 4 cases there was improvement when healing was received concurrently with orthodox medical treatment.

(8) One case examined before and after treatment by the healer gained no benefit and continued to deteriorate.

A child, V.T. (Group 1), was claimed to have responded miraculously to a healer and the name of the hospital where she has been treated unsuccessfully was obtained through the newspaper concerned. The parents were 'adamant in their refusal' to permit the records to be made available.

M. L. (Group 2), reported in the press as having been cured by Mr Edwards, writes: 'I cannot claim to have derived any benefit as a result of that one visit, as that was more than two years ago and it is only in the past few weeks that I have felt any improvement; whether this is due to treatment I have received at the Hospital Clinic, I cannot say.'

Mrs M. H. (Group 2) was the subject of an article in a well-known pictorial magazine; the patient sent *her* history. After several X-ray and anaesthetic examinations the hospital could do nothing more for her; she was discharged, 'presumably as incurable'. She had been obliged to continue wearing her surgical belt for thirteen years and could not get out of bed without it. In 1949 she went to her healer at whose hands she was 'cured'.

The *hospital* records revealed that Mrs M. H. had had an appendicectomy in 1934, and a curettage for cervical erosion. In 1936 there was a barium investigation revealing nothing more than visceroptosis and in 1943 there was a further examination, all with negative findings. Her doctor subsequently wrote in terms which did not substantiate her claims or those of the newspaper and gave his opinion that there was a large functional exaggeration factor.

J. R. (Group 5), according to the headlines, was 'permanently cured'—'psychic healing succeeded when doctors failed'. He was 'given up by professors and doctors who examined him as a hopelessly incurable case. He was born paralysed in legs and arms, he was dumb and he had a distended stomach'. 'After four years he received one treatment and the paralysis left him'—'the next morning he spoke and he could run'—'J. has now grown into a fine young man, leading a normal, happy life.'

In due time the hospital reported that he was an in-patient for two months in 1934, suffering from rickets—discharged improved. From September 1934 to February 1935 he was treated for coeliac disease, chicken-pox and whooping cough and discharged

improved. From December 1948 to February 1949 he was suffering from Brodie's abscess of the ankle and was discharged with satisfactory results. There is no record of any other disability, temporary or permanent.

Mr J. E. E. (Group 3) wrote to Mr Harry Edwards in July 1953: 'In June 1952 after having been on your list of Absent Healing, I suddenly recovered the sight in my right eye which had been completely blind for over 50 years. It came as long sight and was pronounced by my specialist optician as perfect, and the healing as miraculous.' The specialist in ophthalmology is quoted as having said: '(1) Vision perfect; (2) eye clear, bright, in splendid condition; (3) absolutely no fear of deleterious effects, therefore no need whatever of worry.' He was overheard to remark, 'It's miraculous.'

The ophthalmologist concerned was kind enough to write in October 1953, 'There is no miracle—he was a case of spontaneous dislocation of lens which was cataractous. The lens dislocated back into his vitreous chamber which is the old operation known as couching and is brought about by some violent exercise or some sudden jerk—generally forgotten when a miracle is under consideration. Although uncommon it is a well-known clinical entity'. (Could there have been functional blindness after the couching which was relieved by suggestion, the patient beginning to *see* after being encouraged to *look*?)

Mr R. B. (Group 3). Biopsy (operation for removal of a specimen for pathological examination) was carried out in June 1953 and a week later Mr B. was informed that he was suffering from cancer of the larynx demanding a major operation. Mr B. applied for direct healing and during the interview his hoarse voice began to improve and gain in volume. On 21 July 1953 Mr B. was re-examined under an anaesthetic in hospital and informed that the pathologist's report was at variance with the previous one. Independent examination was arranged—'In all Mr B. has been examined by five throat specialists, one of whom is considered to be the greatest authority on cancer in this country.' The two specialists who examined him after he had had direct healing from Mr Edwards both reported 'no cancer now'. One of the surgeons wrote to me in December 1953: 'I doubt if anyone will give a definite reply . . . my own belief is that it was pure fortunate coincidence that this man had a piece removed for biopsy and it happened to contain all of the carcinomatous tissue.'<sup>1</sup>

<sup>1</sup> After this paper had gone to press, a case similar to that of Mr R. B., but not involving any claim to paranormal healing, came to my notice.



Dr O. (Group 4). In large headlines again, the press announced 'Doctor cured in three treatments'—'my own profession would have kept me in hospital for weeks.' Four years previously he had acute pain in the lower back, diagnosed as 'slipped disc', relieved by a month's rest in bed. Two years later he again 'slipped' the disc following a fall. The symptoms were more severe than previously, the least movement causing acute pain, and an emergency appointment was arranged with a healer. 'Without undressing me or even taking off my coat he ran his fingers down the centre of my back over my clothes; reaching the spine he exclaimed, "Ah, here's the trouble, you'll be alright now." Beyond a slight swaying movement there was no attempt at manipulation of any kind. The acute pain left me at once and I got up and walked unassisted up some steps. The dull ache which was all that was left of the pain was cured by two subsequent treatments a few days later. This attack was incomparably more severe than the first. How should I have fared under orthodox surgical methods? I have often wondered.'

Between these episodes a sudden acute pain in the lower abdomen took Dr O. to a surgeon and two other doctors who diagnosed hernia, 'small but definite'; 'I walked with difficulty holding the rupture in with the fingers of my right hand. The surgeon said there could be no cure without operation and I asked for time to consider it. A friend, a scientist, and a Spirit Healer in his spare time, treated me by laying on of hands under Spirit Influence. After four or five treatments the hernia completely disappeared—there has been no trouble since.'

In the first illness described it is obvious that acute pain was the major factor and neurologists and orthopaedic surgeons are familiar with the vagaries of the so-called 'Slipped-disc' symptom-complex. The matter of the hernia seemed at last to afford incontrovertible evidence in favour of the healer. However, this phenomenon of spontaneous 'cure' is known by surgeons to occur in the absence of therapeutic intervention.

D. G. (Group 5) came on to the Festival Hall platform with severe kyphoscoliosis and compensatory torticollis of long standing. Mr Edwards reduced the spinal curvature in his usual way by encouraging the patient to allow himself to move back and forth

Mr X. Y. was examined at Hospital in June 1953 complaining that he had suffered from intermittent hoarseness of voice since the previous February. A small nodule was found on the left vocal cord and at biopsy a diagnosis of early carcinoma was made. Improvement has been steady since that time, and on 6 June 1955 the patient was completely well; 'larynx still remains normal'.

and sideways with the healer's hand supporting. There appeared to be marked improvement in the spinal line, and a mild joke was created by the sight of the jacket which now possessed an unfilled sack where the deformed spine had previously lodged. Meeting D. G. by chance after the demonstration it was possible to talk in comparative privacy among the throng of people too busy to notice him. Commenting on the apparent improvement in his back and on the residual torticollis, I offered to reduce this and did so by placing one finger under his chin and encouraging him to allow his head to move into the normal position, at which point I said he would now remain well and left him. In the meantime I had taken particulars which enabled me to ask for his hospital records. Regrettably many had been destroyed after the usual storage period but there were records dating from 1946 from an orthopaedic hospital disclosing a diagnosis of advanced spondylosis ankylo-poetica ; a Taylor's jacket and supportive physiotherapy had been recommended. In the middle of 1951 he had complained of pain and stiffness and 'would therefore appear to have developed a further increase in flexion deformity in the spine in the course of the last year'. Early in 1952 contact was renewed to enquire after his progress since the demonstration only to find that the effects of Mr Edwards's treatment, and of my own, had lasted about three months, following which he had relapsed. A few weeks later he was admitted to hospital suffering from pulmonary tuberculosis.

This raises a question. Even if the deformities had, in fact, not relapsed, what is the nature of a power which assists in the healing of one disability while laying the body open to invasion by another, more dangerous, illness? What had happened in the disturbed mediastinum? Had the spinal manipulation been accompanied by intra-thoracic trauma?

Mrs G. M. L. (Group 5). A newspaper banner-line stated 'Deformity Melted Away'. 'Mrs G. M. L. who had no power in her hands for about ten years, and whose arms had been crippled with pain, was helped from the stage by a *Psychic News* reporter' who writes, 'She gripped my hand with the strength of a normal person.'

This lady, upon enquiry, wrote in these terms : 'I am sorry to tell you I am not cured of my complaint but, in fairness to Mr Edwards, had I followed his advice and continued with the Psychic Healing at my Local Spiritualist Church I may have been by now. Also, in fairness to Mr Edwards, after his treatment at the Pier Theatre, Hastings, I certainly did feel free of pain ; *for the time* a new woman.'



Miss E. W. (Group 6) was interviewed personally. This lady had, at the age of three and a half years, 'been in splints for a year', during which time she had 'congestion of the lungs'. She had 'spinal trouble' believed to be due to 'falls and tuberculosis'. In June 1949 the patient fell down a steep staircase and although only bruised she 'became more and more tired' and as the months passed the 'back ached where it was deformed'. Later she slipped and fell, following which she was unable to lift her leg; she 'dragged it along suffering agony' but did not attend her doctor as she was 'afraid of having to lie up again'. She went instead to a chiropractor who told her that 'three discs had slipped in the lumbar spine and he thrust them into place'. He 'did not touch the dorsal spine which was deformed for fear of breaking it'. Following this she became worse and 'very bad at the walking', and experienced great difficulty in getting out of a chair or bed. Her own doctor referred her to a surgical specialist and she subsequently received physiotherapy, but the pain and disability increased until she could scarcely turn her head and 'it was awful in a car or bus'. Pain, disability, and faulty gait persisted and in 1950 the patient contacted Mr Edwards and received absent healing. After some months she 'did not get any worse' and was still reporting to the hospital for treatment. Pain and disability continued until mid-May when 'during the night I felt my neck being gripped and pulled and when I got up in the morning I found the top of the spine (which as far back as I can remember, had protruded) had slipped into place and I was able to bend my neck from there which I had not been able to do'. There followed a journey to Mr Edwards who straightened the dorsal spine ('which was certainly a miracle after being deformed for forty-two years'). 'I was able that day to throw away my stick and run into the house. I have no trouble in my balance and can now walk normally.' Writing in April 1952 she says, 'I am hoping to get further benefit.' (Note the contradiction.) This lady, examined by the writer in August 1952, certainly appeared to be a reliable witness. She had visited a healer on the principle of trying anything—'It couldn't do much harm'—and she now had no symptoms apart from 'the back [which] does get a little tired'.

The surgeon originally responsible for her treatment kindly agreed to see her again and report on her condition. His original records revealed that Miss W. had attended the surgical outpatient department between 1949-51. She had a dorsal kyphosis considered probably to be due to an old tubercular lesion of childhood. A possible diagnosis of disseminated sclerosis had been rejected and the spasticity was thought to be associated with the

spinal lesion. She received physiotherapy, a Taylor's brace, and some vitamin injections to relieve the neuritic pains. The report continued: 'One day after I had not seen her for some months she walked in and told me that she had been cured by a Christian Scientist. He had put his finger on the prominence of her spine and she had had no symptoms since. I told her I was delighted that her symptoms had been relieved and she went away. I noted at the time that *there was no obvious change in the physical state*; the kyphosis was as marked as ever.' Re-examining her in 1953 the same surgeon, with the aid of the radiologist, says, 'It would seem therefore, that the physical state is actually the same as it was before she had her treatment, that is, *the organic bone formation is actually the same*. There is no doubt, however, that *her movements are much better than they were before*, due, I suspect, to the psychological improvement.' Obviously there had been *great functional improvement* in this case, but who shall say which kind of therapy or which therapist was responsible?

Mrs L. W. (Group 6) had a history of osteo-arthritis or rheumatism for fifteen years or more, during which time she was unable to walk, her knees being 'enormously swollen'. There was no response to treatment at various hospitals and clinics and at the hands of an osteopath. She was taken to Shere in January 1951 for treatment followed by absent healing. The first treatment provided immediate relief although considerable pain remained; gradually the patient could lift her feet without pain. Describing the treatment by Mr Edwards she commented that he lifted her leg right up without pain, which she 'couldn't possibly have stood if a hospital doctor (or she herself) had attempted it'. She continued to receive treatment from a healer 'for fear of slipping back'. The patient commented that during the several weeks of waiting for her healing interview she received absent healing. One night she was awakened by the feeling that the door had opened and a presence was beside her bed; she felt herself turned over and there was something like a murmur of voices in the room. She fancied herself 'a little easier' next day.

The hospital concerned reported that this lady had, since 1948, been attending for treatment of a non-toxic goitre. In addition she had been treated from November 1950 to June 1951 for benign hypertension, obesity, and osteo-arthritis of both knees, the latter checked by X-ray examinations. There was 'considerable symptomatic improvement' when last seen. The physician wrote in October 1952 to say '*... she seems to have improved subjectively* a good deal since the laying on of hands of her spirit



healer—we can find little change since before this operation . . . the angles of movement are about the same as they were at that time. I think this is primarily degenerative joint disease plus psychogenic pain—a field which is peculiarly susceptible to cure by spirit healers’.

M. R., a boy aged 9 (Group 8), suffering from pseudohypertrophic muscular dystrophy was examined in consultation in December 1951. He later visited Mr Edwards who told him he ‘would get better and that he had straightened his back’. (I had found no spinal deformity.) The family doctor wrote in February 1953, ‘I am sorry to report that in my opinion the condition is very definitely worse.’

The following are phrases from letters from patients referred to the writer by a healer for follow-up of treatment by absent healing. (These belong to Group 7.)

(1) ‘My friend who was to have undergone a serious operation last Tuesday is now *out of hospital* having been told that she has improved so much that no operation will be performed. Thank you for your intercession. . . . Do you think my eye trouble will ever be cured?’

(2) ‘I have spoken to the sister *in the hospital* who told me that she [a girl of six] was dying and the recovery was a miracle.’ (The illness was meningitis, treated in the hospital by modern methods.)

(3) ‘The cancer specialist says she was a walking miracle . . . he had never had a case like it—the spine and later the pelvis which were attacked by cancer have calcified and strengthened in a remarkable manner—thought she would never walk again and gradually through your and the Spirit Doctor’s help the use of her limbs has returned.’ The patient herself a few days later wrote: ‘Dr P. has *not* said I am cured, nor a walking miracle but he was pleased with the way my spine had yielded to treatment. I still have almost as much pain as I had and still have to have frequent injections of Omnopon.’

(4) ‘I was discharged *from the Sanatorium* last Thursday (April 1953) and considering I had a T.B. bowel and both lungs affected I think this is a wonderful achievement. I cannot thank you enough for *all you have done* for me.’

(5) ‘They had a specialist who said it was polio and wanted my niece to let him go into hospital but she preferred to nurse him herself—he had progressed so quickly it must have been his Mother’s wonderful nursing. His Mother says, “No, that is the help that Mr Edwards has given.” The specialist was surprised

at the quick relief from complaints. His speech is normal again and he seems his own bright little self except he has a *slight limp in one leg*. We all feel that the prayers of the Spirit people have helped him.' (Probably less than 15 per cent of poliomyelitis victims suffer from permanent paralysis.)

### DISCUSSION

Some patients investigated, claiming to be improved or cured by a healer after orthodox medicine had failed, were functionally improved although expert medical opinion admits no change in the organic condition. By means of what attribute was this achieved if other than suggestion?

In two cases, a change, rare but not unknown in orthodox medical experience, took place in the organic state soon after treatment by the healer. Was this coincidence?

Thousands, who have given up hope of improvement at the hands of orthodox medical practitioners, either feel better or find new hope after contact with a healer. Is this contribution of value to society? Philosophers, scientists, and priests of various denominations encourage me to continue this search for truth.

The report of the Committee of the Lambeth Conference (13) set up in 1920 states: 'Our Committee has so far found no evidence of any cases of healing which cannot be paralleled by similar cures wrought by psychotherapy without religion, and by instances of spontaneous healing which often occur even in the gravest cases in ordinary medical practice. These facts do not, in our opinion, detract from the good done by many religious healers; and, where the religious influence is of a wise and reasonable kind, greater and more permanent results may be expected than from non-religious methods.' The Church has again set up a Commission to study the problem. Clinical psychiatrists using suggestion as a specific therapeutic technique continue to achieve healing results at least as good as those investigated in this series and, on the evidence to date, it is difficult to conclude that the healer's art is more than an empirical technique.

In the meantime the relevant facts of the body-mind relationship continue to be elicited and formulated in the field of psychosomatic medicine. (For 'medicine' read 'surgery'—for there is no bodily ailment without concomitant or secondary psychic reaction and no change of mental state without a physical component.) The problem of causality has been discussed by Strauss (14) '... we have mind acting on mind as the efficient cause, mind acting on body, body acting on mind, and body acting on body as



*causae agentes*'. It is also important to remember that the 'single pathology' is uncommon in that two or more (apparently) unrelated symptom-complexes may co-exist in the patient and may follow different courses (15).

Strauss's reminder of Mesmer's thesis for the medical doctorate 'on the effects of the planets on the human body', and Gould's dictum (16) 'affect is the mordant of conditioning' throw light on the significance of emotion in suggestion and auto-suggestion.

Clark-Kennedy (17) describes 'most diseases [as] being due to a complicated interaction between genetic weaknesses, the adverse factors . . . in the environment, and the psychosomatic machinery of the individual. . . .'

Murray (18), Irvine (19), and Flind and Barber (20) draw attention to the psychogenic appearance and prolongation or reappearance of 'rheumatic' conditions and Dunbar (21) says 'patients with rheumatic disease differ from all other [psychological] groups'. MacKenna and Macalpine (22) and O'Donovan (23) have pointed the way to the relief of dermatoses via study of the psychological make-up of the sufferer.

Wood (24), Wittkower (25), Woolfe (26), and Hambling (27) have reported on the psychogenesis of cardiovascular disability.

Schoenberg (28) associates primary glaucoma with psychic disturbance and the beautiful composite case-history invented by P. W. Brown of the Mayo-Clinic and mentioned by Weiss and English (29) provides a lesson for all clinicians.

More recently O'Neill, Reading and Malcolmson (30) and Capps and McNab (31) and Bellak (32) have investigated the psychological factors in chronic vasomotor rhinitis and allied conditions.

Bellak also comments on the personality of the doctor—'What the doctor does and the way he does it is determined not only by his scientific skill and knowledge but also by the way he is affected by the patient.'

Hundreds of thousands, possibly millions, of sufferers seek unorthodox aid and it is important to assess whether, in general, the healer is a power for good or harm. Even if many who have not been helped by qualified doctors are given hope, does the healer, by his intervention, prevent a significant number of patients from receiving adequate medical care in good time?

There remains a wide unexplored field of enquiry, and much individual case-work in different racial and social groups is necessary, first to establish the facts of paranormal diagnosis and treatment, and then to correlate their relationships and significance. This field-work might usefully include a review of cases in which improvement has occurred after the cessation of medical treatment

without the intervention of a healer. Long-term controlled experiments could be devised. It is possible that the exploitation of suggestion in rehabilitation following disability of all types might prove to be a valuable adjunct to normal therapeutic approaches and it may well be that the attitude of doctor to patient (or to disability) is of far greater significance than has hitherto been recognized, except in the psychoanalytic 'counter-transference' situation.

Case-histories apparently substantiating the claims of the healer should be investigated by a suitable panel with access to the opinions of sub-specialists, since the accumulating evidence of body-mind relationship constantly reveals new psycho-physiological rationale in hitherto inexplicable cures. Williams (33) exhorts us 'in medicine [to] expect the unexpected—but be surprised if it occurs'.

Thanks are due to Mrs K. M. Goldney, Organizing Secretary of the Society for Psychical Research, at whose suggestion this enquiry was undertaken, for much new case material and other most valuable help; also to Mr Harry Edwards, doctors, patients, Hospital Records Officers, and others who have made this survey possible.

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## REPORT ON THE OLIVER LODGE POSTHUMOUS TEST

ON 20 June 1930 Sir Oliver Lodge deposited a sealed packet with the Society for Psychical Research containing the posthumous message which he hoped to transmit through a medium after his death. Sir Oliver died in August 1940. Owing to the war the packet was sent out of London for safety with all the relevant documents and was not returned until the end of 1945. A committee was formed in 1946 consisting of representatives of the S.P.R., the London Spiritualist Alliance,<sup>1</sup> and members of the Lodge family, with Mr George Tyrrell as Chairman to deal with what promised to be a most complex test.

The packet consisted of seven envelopes, one inside the other, with his message enclosed in the innermost envelope. The six preceding envelopes contained instructions regarding the procedure to be followed by the sitters, and certain 'clues' which

<sup>1</sup> The name has lately been changed to the College of Psychic Science (C.P.S.).

were to be given by degrees to mediums and were intended to stimulate and guide Sir Oliver's memory towards the final message.

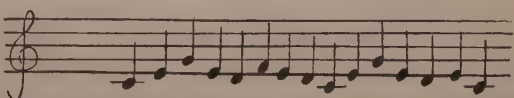
In addition to this packet of envelopes a 'supplementary envelope' marked 'B', containing another, was sent to the S.P.R. on 23 December 1931 and yet another marked 'C', containing an inner envelope, on 10 October 1933, making a total of 11 envelopes at the S.P.R.

Another sealed envelope containing 5 envelopes was deposited at the L.S.A. in November 1931. These were also to be opened seriatim and contained further clues to aid his memory. In the innermost of these envelopes the main message (given in the final S.P.R. envelope) was repeated with additional explanations.

The final message chosen by Sir Oliver as evidence of his identity and survival, dated 10 June 1930, consisted, in his own words, of the following :

'An elementary exercise which has more or less been an obsession. I have never written it out before, but I have strummed it on tables and chairs thousands and possibly a million times. Never so as to be noticed, and often so as to get finger touches to correspond with the frequency of contacts here. Thus the thumb strikes 3 times, the third finger only once, and the fourth or little finger twice. Consequently, by striking the whole of the fingers on the table, then raising the third and striking again, then the fourth and strike again, all would have struck and been satisfied except the second or middle finger which would have to strike three times more.'

This five-finger exercise was given in full as follows:



(a) (the notes)

(b) (alphabetical names) c e g e d f e d c e g e d e c

(c) (the fingering) X 2 4 2 | 3 2 | X 2 4 2 | 2 X

(d) ('sol-fa' names) Do, mi, sol, mi, re, fa, mi, re, do,  
mi, sol, mi, re, mi, do

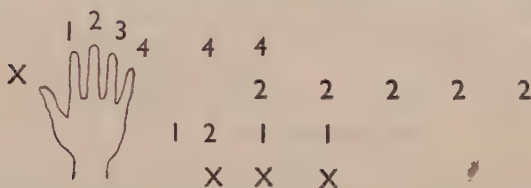
Sir Oliver wrote, in notes accompanying the final message, that the exercise was invariably linked in his mind with the number of contacts made by each finger, and that his idea throughout life (never formulated) was to play the exercise as many times as possible. There were 15 notes, and the fingers made 3, 3, 6, 1, 2,



hits respectively, i.e. 15 in all. Sir Oliver also alluded to a habit of using a 'shorthand' method of playing the exercise, partly in chords instead of single notes, which quickly gave him the same number of finger contacts, i.e. 15. This enabled him to play it more often. (Note: details of this method are given in S.P.R. envelope B. 2. below.)

An additional habit was to omit the third finger. This he illustrated with the following diagram:

X 2 4 2 | 2 | X 2 4 2 | 2 X



[Note: Total contacts here, 14.—K.G.]

He wrote, 'This is a side issue, but I may mention it, and therefore explain it here. Most likely it will not be referred to at all.'

This concludes the summary of the final message.

In order to evaluate the sittings containing statements which apparently refer to items connected with the test, it is necessary to study the 'clues' intended by Sir Oliver to stimulate his memory, and also the dates on which the envelopes containing them were opened.

*S.P.R. envelopes Nos. 1 and 2;*

*dated 10 June 1930; opened 10 February 1947.*

'It is not an incident or an episode and not possible to be guessed.' 'The subject matter is exceedingly trivial.'

*S.P.R. envelope No. 3;*

*dated 10 June 1930; opened 14 April 1947.*

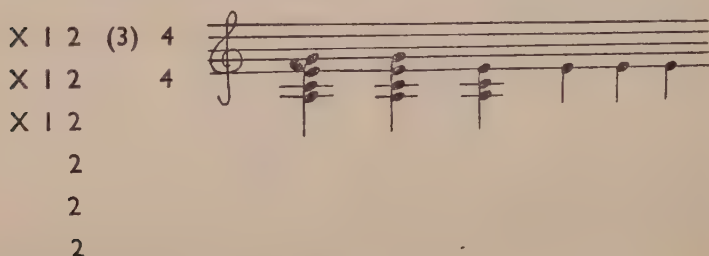
... something that trivially rather obsessed my life ... that I have never mentioned to anyone.'

*S.P.R. envelopes B.1 and B.2;*

*dated 23 December 1931; opened 23 February 1951.*

B.2. contained the following 'shorthand' method, together with the following message:

This gives the 15 notes, not in sequence but in shorthand. The F can be omitted, and the rest of the finger contents can be repeated again and again. . . . This is a third or supplementary record in case I mention hereafter that there was a simultaneous way of quickly reproducing the notes. Here is another version of it :



This bears out the 3, 3, 6, 1, 2

(Note : It should be noted that the number 3 in the fingering is bracketed in order to remind Sir Oliver of his occasional habit of omitting this finger playing the F, as explained in the final envelope. The above exercise enabled Sir Oliver to retain a 'finger memory' of the main message although the notes were not identical with it.—K.G.)

*L.S.A. envelope No. 1 ;*

*dated November 1931 ; opened 7 April 1951, contained*

*L.S.A. envelope No. 2*

on which was written : 'It may be partly a number or it may be a more definite reference to some childish recollection' . . . 'Psychometrists may try their hand at it . . . ' The writing on envelope No. 2 also alluded to 'the S.P.R. packet which contains 5 or 6 envelopes to be opened seriatim. This contains only three I think . . . '

*L.S.A. envelope No. 2 ;*

*dated November 1931, opened 3 December 1951, contained*

*Envelope No. 3*

on the outer cover of which was written, 'This envelope may be opened without much compunction if any numerical message has been supposed to come through from me' and on the back 'It is to be used in conjunction with the S.P.R. document and mainly refers to a possible numerical communication . . . ' Also, 'I could wish that this had reference to something classical or recognised, like for instance Schumann's Songs of Childhood, Opus 15, or that it referred to an extract from some other such piece of music. But



all I know is that it was the first simple and easy exercise in a book used by teachers in the fifties or sixties of last century . . . and was probably nothing more ; though the book itself doubtless contained extracts from real compositions. Why I remember it so well I cannot tell . . . but as I do remember it I utilize the accident and try to make use of it.'

*L.S.A. envelope No. 3 ;*

*dated 25 November 1931 ; opened 22 February 1952*

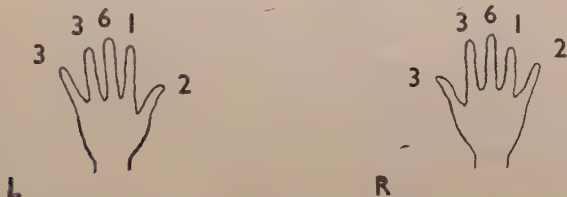
contained the following clue : 'If I give a number of 5 digits it may be correct, but I may say something about 2 8 0 1 and that will mean that I am on the scent. . . . It is not the real number (which is written in the inner envelope) but it has some connection with it. In fact it is a factor of it.'

(Note : It will be noticed that  $2801 \times 12 = 33612$ .—K.G.)

*L.S.A. envelope No. 4 ;*

*dated 22 November 1931 ; opened 16 March 1953*

contained the following diagram and a letter in which he says 'clearly the number to be given is 3 3 6 1 2, and it has something to do with fingering. That is as far as this envelope will take you.'



Sir Oliver evidently assumed that the sitters would be in free communication with him through some medium and would give him a 'stimulus' hint from an envelope in the series and receive information from him regarding the contents of the next envelope and be told when it should be opened. Numerous sittings were held by the members of the Oliver Lodge Posthumous Test Committee with various mediums, but these conditions were never fully realized, and no clear unambiguous information obtained concerning the contents of each envelope before opening. However, messages were received from time to time, purporting to come from Sir Oliver giving instructions to 'open further' and saying that part of the message had already been given though unrecognized by the sitters at the time.

The subsequent envelopes at the S.P.R. (Nos. A. 4, 5, 6, and C.1 and C.2.) which preceded the final envelope (A.7) were found

to contain no further hints, but repeated some of the previous clues and gave additional explanations. On 19 May 1954 these envelopes, together with the final S.P.R. envelope No. 7, were opened by Mr W. H. Salter, and the final L.S.A. envelope No. 5 by Brigadier Firebrace at 147 Victoria Street, London, S.W.1, (the home of Mrs William Sitwell). There were present the following representatives of the S.P.R., the L.S.A., and members of Sir Oliver Lodge's family :

Miss H. Alvey	Mrs Hankey
Major Dampier	<sup>1</sup> Mr and Mrs Brodie Lodge
Brigadier R. H. Firebrace,	<sup>1</sup> Miss Norah Lodge
C.B.E.	<sup>1</sup> Miss Mercy Phillimore
<sup>1</sup> The Hon. Mrs C. H. Gay	Mr W. H. Salter
Mrs K. M. Goldney, M.B.E.	Mrs William Sitwell

From 1947 onwards about 130 sittings were held by members of the Committee with various mediums, and over 100 automatic scripts and letters were received from other persons purporting to give the message. Before considering the results of these communications, the following points should be remembered. In his statement deposited with his first packet at the S.P.R. on 20 June 1930 he said that 'the message is detailed and could not be guessed at any of the stages'. This gives the impression that although the 'hints' would revive his memory they would not act as clues to the sitters. However, it turned out that the main clues were in fact part of the final message, which was therefore being partially divulged in the preceding envelopes (both S.P.R. and L.S.A.) although the sitters were unaware of it. This increases the difficulty of judging the value of statements made by mediums *after* clues were known to the sitters, owing to the possibility of telepathy from sitter to medium. In short, by December 1951 the sitters had been told by Sir Oliver's written 'hints' that the message was:

'Something that trivially obsessed his life and was never mentioned by him to anyone' . . . 'not an incident or an episode'; that it was connected with 'music and fingering'; 'the numbers 3, 3, 6, 1, 2' (= 15 which represented the number of finger contacts); the notes of the 'shorthand' method; a 'possible numerical communication'; 'a childish recollection'; and that it was 'the first simple and easy exercise in a book used by teachers'.

<sup>1</sup> Members of the Oliver Lodge Posthumous Test Committee, which included Mr G. N. M. Tyrrell and the Rev. C. Drayton Thomas, now deceased.



The only *unknown* facts remaining to be revealed were :

(a) That the 'obsession' was the habit of strumming a passage of music silently on tables and chairs.

(b) The actual notes of the five-finger exercise and their sequence and fingering.

(c) Their names, both alphabetical and by the 'sol fa' method.

(d) The occasional habit of omitting the third finger in the shorthand method alluded to in S.P.R. envelope B.2 ('The F can be omitted') but not explained until the final envelope.

In S.P.R. envelope 'C' (opened on 19 May 1954 at the same time as the final envelope) Sir Oliver wrote the following remark about the test: 'As an effort of memory it is worthless unless it serves the purpose of excluding telepathy and of proving my identity hereafter.' We are therefore confining ourselves to sittings containing some apparent references to parts of the final message which cannot be attributed to telepathy from the sitter. Before quoting extracts it should be emphasised that it is essential to read the sittings in full in order to avoid giving a false value to sentences taken out of their context. It is easy to give undue or insufficient significance to an isolated phrase.

What evidence is there of any medium having produced any of the details of the final message, or of the 'clues' in the earlier envelopes before they were read by the sitters? No one gave the complete message ; but one medium referred to part of the final message without knowing any of the clues, and the details to which he referred were in the final envelope and unknown to the sitters. Two sittings were held with this medium.

#### MEDIUM: **MR JORDAN GILL**

8 May 1951. Sitters, Mr Tyrrell and Miss Phillimore, with wire recorder (envelopes S.P.R. Nos. 1, 2, 3, and B.1, 2, and L.S.A.1 already opened). Mr Tyrrell was aware of the reference to music and numbers in B.2, but Miss Phillimore was only aware of the clue re 'a trivial obsession' and 'not an incident or an episode'.

The L.S.A. packet was used at this sitting and given to Mr Jordan Gill to hold. He was not told the object of the sitting, but as Mr Tyrrell was known to be the Chairman of the Oliver Lodge Posthumous Test Committee and had published a report in the S.P.R. *Journal* in September 1948, it is not impossible that Mr Gill may have guessed his purpose. It must be remembered that the envelope L.S.A.2 containing the 'clue' referring to 'a

simple easy exercise in a book used by teachers' had *not* then been opened, and the alphabetical and 'sol fa' names were only disclosed in the final envelope on 19 May 1954 and were known to no one.

At this sitting a description was given of matters mainly relevant to Mr Tyrrell or applicable to either Mr Tyrrell or Sir Oliver. The medium then continued as follows:

? *Re. the Five-finger Exercise (item (b), p. 127).* 'There is also a clear impression of music—I feel Italy strongly—now I seem to be sitting at a piano, seem to be going through all the *five finger exercises* building up (missing word) everything all being given me here at the present moment—music.' (Note: Sir Oliver visited the memorial to Frederic Myers in Rome a few months after depositing his sealed packet with the L.S.A.—K.G.)

15 June 1951. Sitter: Miss Phillimore (with wire recorder). (No further envelopes opened.)

? *Re. Alphabetical and 'sol fa' names of notes, etc. (item (c), p. 127).* At this sitting Mr Jordan Gill was told that he was taking part in the O.J.L. Posthumous Test and that the packet given to him to hold was the one deposited at the L.S.A. by Sir Oliver before his death. Mr Jordan Gill began by giving a description of Frederic Myers and was then asked by the sitter to turn his attention to the packet and give any impressions he might receive, to which he replied at once: 'I have got here symbols and hieroglyphics that in some way convey the *alphabet* to me. I can certainly see a jumble of letters and yet in some particular respects I have also got an indication of I think one might say Roman numerals . . . it seems to me it's got to point to a *series of letters in the alphabet and they form a chain of some description, and these letters seem to spread out, not so much in initials as in a code.* Alphabet. I must stick to the alphabet.

' . . . Yet I must bring in also—there is a certain *Latin term* here that I can't quite get and it seems as though I suppose the Roman numerals are also linked with that . . . there is a book would link with this somewhere. *These clues will point to a book.* Within the book will be found certain information all of which will lead to the actual contents—I feel somewhere or other if only I can have the bits and pieces of the alphabet here I could so fit them together that they would probably lead to some not so much names as code, that would direct one's footsteps, and it would be a link establishing something or someone in the body as well as someone in the spirit: but more important it would be a link between the two worlds.'

No reference was made to any other items.

(Note : The line of numbers giving the fingering are not Roman numerals. The 'sol fa' names are the first syllables of words used in a Latin phrase to be found in the Office hymn for the Nativity of St John the Baptist, and were chosen by Guido d'Arezzo when he introduced this method of singing at sight in the eleventh century. The L.S.A. packet given to the medium to hold at this sitting did *not* contain the 'sol fa' names, which were only disclosed by Sir Oliver in the final S.P.R. envelope, then at the Society's office.

Psychometry cannot be entirely excluded as a possibility in this experiment, but cannot be said to apply to the 'Latin term'.

The allusion by Mr Jordan Gill to a book, etc., might be appropriate to the book of five-finger exercises.

This sitting with Mr Jordan Gill is the only sitting containing reference to the final message which had not been mentioned in any 'clues', was unknown to any living person, and had not been given by any other medium. The statements were made in direct reply to the sitter's request that he should see if he could receive any impressions from the sealed packet which he was given to hold. There was no question of his reply referring to anything else, nor did he attempt to interpret his impressions, which he evidently could not understand.

There were various allusions in this sitting to events in Sir Oliver's life, but as these had already been published and had no connection with the test they are not evidential.—K.G.)

#### MEDIUM: MISS G. CUMMINS

The next medium to produce information regarding the contents of the final envelope was Miss Geraldine Cummins ; but in this case all the 'clues' were known to both medium and sitter. She wrote two automatic scripts on 12 May 1954, a week before the final envelope was opened. Mrs Gay was present during the writing of the first script.

The result was as follows :

12 May 1954. Sitter: Mrs Gay. (All envelopes containing clues already opened and known to both medium and sitter.)

? *Re. Silent Strumming (item (a), p. 127)*. Miss Cummins is the only medium who stated quite definitely that the 'trivial obsession' to which Sir Oliver alluded was that of playing a tune silently with his fingers when he was alone and that it dated from his childhood and had persisted all his life. She began by describing Sir Oliver's very early childhood and his love of making patterns with bricks when listening to piano-playing and his



association of Schumann's Opus 15 with numbers as he grew older. She wrote that owing to a sudden fright caused by a crash of broken china an impression of a peculiar kind was made on him.

'I started playing with my fingers, a nervous mannerism you might call it; but it sprang from this upset to me that afternoon. I did not drum or play with my fingers when with people. It was when I was faced with some difficult problem in connection with my work that I played without a piano. No notes were touched, you understand, but it gave me a curious and rather guilty pleasure to start playing. Opus 15 especially. I do not think it was good for the particular problem or work I was studying. It side-tracked me, prevented me from going on with it, and that somehow eased my mind. I became, as lessons grew more difficult, enamoured of this five-finger exercise,  $3 \times 5 = 15$ .

'It started me off. Then I seemed as it were to think more easily. On the other hand this playing with my fingers inevitably became a practice when I was alone and about to start work. I made up my mind I would try to control it but I had difficulty in this respect and certain chords would insist on breaking up concentration.'

(Note: The 'clue' re Opus 15 was read to Miss Cummins during the sitting and evidently misled her. It is incorrect that playing Opus 15 became a special habit. It is, of course, impossible after so many years to confirm the details of Sir Oliver's early childhood.—K.G.)

? *Re. Numbers (item (b), p. 127)*. She wrote: 'I connected certain numbers with what I played'; but she did not link the numbers with the finger contacts and the fact of a connection between numbers and music had already been disclosed in the clues.

? *Re. the 'Shorthand' Method (item (d), p. 127)*. She did not mention the 'shorthand' method, but she alluded to 'the scale of C. major' and said 'there was a change in piece from one time to another; also a change in key.' No mention is made by Sir Oliver of a scale, but the five-finger exercise is in the key of C major and the 'shorthand' passage mentioned in the S.P.R. envelope B.2. (also played silently by him) is in the key of A. minor.

(Note: In view of the fact that the clues about numbers, music, and fingering were read to Miss Cummins in trance, and that she knew the final message was 'something that trivially obsessed him since early childhood' could she possibly have inferred subconsciously what it was? An experiment was carried out with five strangers who knew nothing about the test and they were given

the clues and asked if they could guess what was 'the trivial obsession', with the result that two of them guessed correctly.—K.G.)

### MEDIUM: MISS THIRZA SMITH

One other medium, Miss Thirza Smith, produced a partial reference to the contents of the final envelope after both she and the sitter knew all the important clues.

28 March 1952. Sitter Miss Phillimore. (Envelopes S.P.R. 1, 2, 3, B.1, 2, and L.S.A. 1, 2, 3, already opened and clues re numerical communication 'simple easy exercise', 'Opus 15', '5 digits', etc., known to both sitter and medium.)

? *Re 'sol fa' names (item (c), p. 127).* Miss Smith sang a passage of ten single notes in C major with their sol-fa names to Miss Phillimore, having heard them on waking from an after-lunch sleep before the sitting. The passage was not identical with the five-finger exercise in the final envelope but the first three notes were correct (C, E, G) as follows:

DO MI SOL TE LA SOL MI RE SOL SOL



1 April 1952. Sitter: Miss Phillimore. (No further envelopes opened.)

Miss Smith again attempted to sing a short passage in 'sol fa' beginning with the notes 'do mi sol', but the rest was inaccurate. Miss Smith is the only medium who actually used the 'sol fa' names to describe the notes; but it must be remembered that she then knew the 'clues' re music. It should also be mentioned that she had learned singing and was quite familiar with the 'sol fa' names, although of course their significance was unknown to her at that time. She made no reference to music at any sitting before 30 January 1952 when the clue re 'Opus 15' and 'a simple easy exercise in a book used by teachers', etc., was read to her.

A long series of sittings was held with Miss Smith in 1951-2 during which she repeatedly attempted to give numbers. On 3 December 1951 envelope 2 (L.S.A.) was opened and a reference to a 'numerical message' written on the outer cover of envelope 3

was read aloud at the sittings. After this date attempts to give groups of numbers became marked, and on more than one occasion included the five digits mentioned in S.P.R. envelope B.2, though in wrong order and in conjunction with irrelevant plus and minus signs. As Miss Smith had not been tested for a possibility of her having 'preference numbers', and as telepathy could not be excluded owing to one of the sitters knowing the correct group of numbers, these sittings are not included in this report.

### MEDIUM: MRS JEAN THOMPSON

*18 July 1951 and 25 October 1951. Sitter: Miss Phillimore (with wire recorder). S.P.R. envelopes A.1, 2, 3 B.1, 2, and L.S.A. 1 already opened.*

The clues re 'a trivial obsession' and 'it may be partly a number or a reference to some childish recollection' were known to the sitter but not the clue in S.P.R. envelope B.2. containing the first reference to music which had been opened at the S.P.R. but not disclosed. The medium was not told the purpose of the sittings and the L.S.A. packet was given her to hold concealed in a blank envelope. She said: 'My first impression is of a picture, and I see writing. Could I not with this envelope, or the contents, travel quite a distance? And I am being impressed to travel where water is. I see here the symbol of animals. I want to record *music*. I see the initial "H".' She continued with a description of Sir Oliver and also gave a number of symbols (a bell, a key, a castle) but nothing relevant to the test.

*25 October 1951 (no further envelopes opened).*

The medium on being given the packet to hold evidently identified it at once with Sir Oliver. She alluded to '*five letters* and we want to complete a word'. (Note: The word appeared to be 'STONEHENGE').

Later on towards the end of the sitting she said:

'He seemed to write and write. I get as if we write a great deal. Are there five letters here?'

*Sitter: 'Five letters in that packet, do you mean?'*

*Medium: 'Yes.'*

*Sitter: 'I don't know. There's only one packet in this envelope.'*

*Medium: '... we feel there is a seal here unbroken....'*

*Sitter: 'There is a seal unbroken.'*

*Medium: 'And I wanted to put up my hand with five fingers. A very definite word is conveyed here.'*



*Sitter* : 'You are making the medium hold up her right hand with fingers spread and the palm upwards.'

*Medium* : 'That is right. Will you take note I do not know if someone tried to make an impression of the hand, but I seem to put my hand down and I am being impelled to do it by the gentleman who has just left. I see him put his hand to his head in this way—stroking his forehead.'

(Note : There were five envelopes in the L.S.A. packet, but only one had then been opened. On the outer cover of L.S.A. 2, Sir Oliver had written, 'This contains only three I think.' The existence of the five envelopes must therefore have been known to the sitter, but the packet was inside a blank envelope from which the medium did not remove it.

It may be claimed that the image of the medium holding up her hand with fingers spread and her emphasis on being impelled to put her hand down was a reference to the five-finger exercise. On the other hand her question about the 'five letters' (which at the beginning of the sitting she definitely connected with a word) seems to indicate that she was referring to the five letters in the packet and has nothing to do with the test message. It was widely known that Sir Oliver had left a series of envelopes as the whole scheme had been described by Mr Tyrrell in his report published in 1948.—K.G.)

There is no doubt that Sir Oliver's test was extremely difficult to fulfil. He evidently realized this fact and in S.P.R. envelope 'C', opened at the same time as the final envelope, he said : 'The memory is chiefly in my fingers, hence I am doubtful if it is a memory that can be reproduced.'

The object of so complex a test was to insure that any 'communication' (to use the word without prejudice) which gave the complete solution could not be reasonably attributed to either chance or normal inference on the part of the medium, and to exclude as far as practicable telepathy from sitter to medium.

It was, however, probable that such a design would produce several 'communications' which neither gave complete solutions nor could be summarily dismissed as complete failures, and left room for considerable difference of opinion as to the degree of success or failure to be assigned to each. We have therefore thought that the best course was to set out summaries of those 'communications' which seemed to us most worth consideration having regard to the normal knowledge about Sir Oliver's test possessed at the time by the sitters and mediums concerned, and to leave it to each reader to make his own evaluation.

We should like, in conclusion, to express our warm thanks to all the mediums and automatists who have participated in the experiment.

17 May 1955

SIGNED

*For the S.P.R.*

KATHLEEN GAY

W. H. SALTER

R. H. THOULESS

*For the C.P.S.*

R. H. FIREBRACE

MERCY PHILLIMORE

CONSTANCE SITWELL

## ESP TESTS WITH EROTIC SYMBOLS

### CORRECTIONS

WE regret some mistakes in the tables given on page 3 of the *Journal* for March 1955. The results were clearly significant, and none of the conclusions in the paper is affected by the corrections, but we would like nevertheless to have them on record for the sake of preserving strict accuracy.

Table II was intended to show that the subject scored significantly on the male and female targets and not on the others. Wing Commander N. Young has pointed out that the figures for chance expectations are wrongly given, no allowance having been made for the fact that the subject called some symbols more than others. Moreover, as Mr A. T. Oram has pointed out, the use of normal distribution tables for evaluating odds gives a somewhat exaggerated result when the sample is as small as 48 trials. However, our point is valid and can be demonstrated in other ways.<sup>1</sup> Corrected and condensed, Table II appears as follows :

TABLE II (revised)

	<i>Erotic Targets</i>	<i>Neutral Targets</i>
Observed no. of hits	39	30
Empirical expectation	21.59	26.39

Is the difference in the frequency of hits on the two kinds of targets significant? On the assumption that the guessing in the two categories followed two independent binomial distributions, and that within each category the probability of a hit was approximately equal for each target, one obtains the following figures :

<sup>1</sup> We are indebted to Mr Robert Radburn for the evaluation which follows.

	<i>Erotic Distribution</i>	<i>Neutral Distribution</i>
Expectation of hits	21.6229	26.4152
Standard error	4.0991	4.6407
Standardised normal deviate $\left(\frac{O - E - \frac{1}{2}}{S.E.}\right)$	4.1172	0.6647

Standardised normal deviate of difference = 2.4413  
 $P = .0073$

The difference is significant.

When the figures in a  $\chi^2$  table are small (say less than 100) it is advisable to apply the Yates continuity correction. This should have been done in the case of Tables III and IV. In addition, there has been a miscount of one in Table III. The amended table is as follows :

TABLE III

<i>Subject's Calls</i>	<i>Targets</i>		
	<i>Erotic Symbol</i>	<i>Neutral Symbol</i>	<i>Total</i>
Erotic Symbol	58	49	107
Neutral Symbol	39	94	133
Total	97	143	240

$\chi = 14.2$  (with continuity correction applied)

As originally stated,  $P < 0.001$  and odds  $> 1,000$  to 1. Applying the continuity correction to Table IV,  $\chi^2$  becomes 5.53,  $P = 0.019$ , and odds = 50 to 1.

Finally, in the Appendix (p. 6), there is a copying error. The twentieth target in run number 7 was square and not star.

INTERPRETATION OF THE RESULTS

It will be remembered that it was only after the subject had explained to the experimenter his erotic associations to certain symbols that he began to score significantly on these particular targets. In the course of correspondence with G. W. Fisk, Dr J. B. Rhine raised an interesting point in this connection. He wrote :

If your subject were responding to the potentially erotic symbols mainly because they matched an outstanding interest, he should have



scored well on them in the first place in your preliminary test. When, however, you made the targets something special on discovering his interest, you did something more and introduced the factor of enhanced motivation with respect to the test itself. Not only that, but apparently you channeled the interest in the test specifically enough to lead him to score effectively on the selected symbols without allowing any spread to the ESP test as a whole. I think it could have been so managed, perhaps, that the excited interest in the test by way of the symbols could have been allowed to spread considerably if not evenly over all the symbols; that is, to making a high score on each run regardless of its distribution. It was, however, more natural, or so it seems to me, for the subject to consider that in playing up two symbols to confine his concentrated attention to them. He did, as you pointed out, scatter his aim noticeably from one to the other of the two, but that was all.

The point, then, that I am trying to single out, is that it is not a question of emotional interest in the target; it is a motivated attention to the use of the targets that counts.

Without further experiment it is impossible to choose between various possible hypotheses as to what caused this subject suddenly to produce the scores he did. Dr Rhine may be right that it was a matter of generally enhanced motivation. On the other hand, it looks, on the face of it, more like a specific interest in the erotic symbols only. Possibly the emotional associations of the 'erotic' symbols were not so definite and clear cut in the subject's mind until after he had explained them to G. W. F. and after the latter had heightened and focussed the associations by preparing the special set of cards.

G. W. FISK  
D. J. WEST

## CORRESPONDENCE

### RANDOMNESS: THE BACKGROUND, AND SOME NEW INVESTIGATIONS

SIR,—From the title of Mr Fraser Nicol's article in the current issue of the *Journal* I hoped that at last I was going to learn something about this mysterious quality of 'randomness' which enters so constantly into the discussion of ESP tests. Unhappily I was disappointed. Mr Nicol contented himself with the usual frank but unhelpful admission that he didn't know what randomness was.

My justification for this letter is that I do at least know what 'random' means. So, I imagine, do all the rest of your readers. A random action is one which is performed without purpose—

without precise aim—with no intention of achieving a specific result. When during the late war it was reported that bombers had dropped bombs 'at random', no-one (except the German commentator who thought it was the name of an English town) had any difficulty in understanding what was meant.

The important thing to notice is that the 'randomness' is inherent, not in the action itself, still less in the result of the action, but in the mind of the actor. Hence (disregarding what little we know of animal psychology) the word can only be applied to a human action. A 'random result' can only mean 'the result of a random act by human agency'. There is no randomness in the result itself, but only in the means by which it is brought about.

The meaning of 'random sampling', therefore, is quite clear and precise: a process of selection by human agency in which the intention to choose a specific sample in preference to another is absent. But there can be no such thing as a 'random series of digits'; a series, *per se*, is incapable of forming an intention. The phrase can only legitimately be taken to mean 'a series aligned by human agency with no intention of incorporating any specific order into the arrangement.'

But this meaning will not meet the needs of the statistician. The arranger may not mean to incorporate order into his arrangement, but that does not exclude the possibility that the order may be there. At the best, random action can only give chance results, and a chance arrangement of the first nine digits is just as likely to yield the order 1, 2, 3, 4, 5, 6, 7, 8, 9 as any other. What the statistician really wants is a 'haphazard' arrangement, if we understand by that an arrangement lacking in any sort of system or order. But there is no such thing. In the last analysis, 'haphazard' can mean no more than 'lacking in any *apparent* order'; for every finite series of digits must exhibit some sort of order, even though it is only apparent to the eye of the skilled mathematician, after prolonged analysis. Even a series of infinite length may display order: a repeating decimal, for instance; while the irrational numbers, which when expressed as decimals can be prolonged to infinity without ever repeating, can still be expressed in other ways which reveal their underlying order. The mere fact that the so-called 'random tables' are amenable to 'tests for randomness' is a demonstration that they do possess order of a kind.

The truth of the matter is that the statistician does not really care what sort of order is possessed by his 'random arrangements', provided that it is some kind of order which will not vitiate his

results. The only true inherent quality that he looks for in the 'random tables' is the quality of yielding consistent results when employed in statistical analysis ; and he finds it there, not because it is a mysterious and all-pervading property of nature, but because it is purposely incorporated in the tables by their compilers. The tables are not compiled by any random process, but in a perfectly systematic manner ; and when completed they are subjected to tests to see whether they will serve their purpose. If they pass the tests, well and good. If they do not, they are either adjusted until they do, or else they are rejected. Like other man-made things, they do not pretend to be perfect, and it is not in the least surprising that Mr Spencer Brown has been able to detect imperfections in some of them. But the conclusions of Mr Spencer Brown have not the least relevance to any ESP results based on other tables, or on no tables at all, or even on the same tables employed in a different way. It is a fact, which Mr Nicol might have stressed far more strongly than he did, that the only way in which Mr Spencer Brown can cast doubt on the results obtained by Dr Soal with Mrs Stewart, for example, is by applying his methods to the actual target lists that Dr Soal employed. If he can produce fortuitous results of the same order of probability (more than  $10^{70}$  to 1) then we might as well drop the whole inquiry into ESP and kindred subjects ; if he cannot, then he might as well drop his. All he has achieved so far is to show that certain not very important and not very well-authenticated phenomena may just as well have been due to imperfections in the material employed as to any other cause ; he has *proved* nothing—except the unreliability of the tables when employed in ways which their compilers did not contemplate—a possibility which the compilers have always admitted, anyway.

Is it not time that we endeavoured to throw off this obsession with statistics? What a preposterous notion is that of Mr Nicol's, that haphazard arrangement is an 'inescapable requirement' of ESP experiments! For the majority of tests it is essential that the order of the targets should be *unknown to the subject*; but haphazard arrangement is no more than a device for facilitating statistical analysis of the results, and statistical analysis in its turn is little more than a means employed to decide whether the results are due to chance or not. But Dr Soal has shown, with a degree of certainty seldom equalled in the history of science, that the ordinary ESP phenomena are *not* due to chance, and his proof applies with equal force to every cognate series of experiments. We need not repeat it ad nauseam. Pasteur proved that life could not arise spontaneously in sterilized media ; the housewife does not have



to repeat the whole of his life's work every time she bottles a couple of pounds of fruit. We *know* now that the phenomena we investigate are real and not imaginary. That is something, truly ; but it is little enough, in all conscience. Let us get on with investigating the phenomena themselves, and stop flogging that dead ass, Chance.

Horning, Norfolk.

N. YOUNG  
*Wing Commander*

SIR,—For Wing Commander Young's courtesy in allowing me to read his letter before publication, I extend my thanks.

The nature of statistical randomness has been a subject of controversy for many years. Some of the leading thinkers are R. von Mises, Keynes, Broad, Reichenbach, and more recently the Cambridge philosopher G. H. von Wright. All of them write with scholarly restraint. There are the 'Buts', and the doubts, the construction of postulates, deductions made from these and so on. Over-all there is the unvoiced 'This is my standpoint, but I may be wrong'. Wing Commander Young vaults all the difficulties by the device of ignoring them, and says, 'I do at least know what "random" means' ; and so strong is his personal conviction that he believes all *Journal* readers agree with him.

The *Shorter Oxford Dictionary* has half a column on the varied meanings of 'Random', and none is mathematical. Wing Commander Young has chosen a definition (or definitions) from the market-place, and it won't do, either theoretically or practically.

Wing Commander Young defines 'a random action' as 'one which is performed . . . with no intention of achieving a specific result'.

As a definition this is not a very happy effort, for its vagueness deprives it of utility. I pass to the fourth paragraph, where again no doubts are allowed to affect the writer's assurance. 'The meaning of "random sampling", therefore, is quite clear and precise : a process of selection by human agency in which the intention to choose a specific sample in preference to another is absent', and so on. This is the kind of observation which may astonish the mathematician, but to the non-mathematical reader it may well have a look of plausibility. Yet it was rejected in the Society twenty years ago. For Wing Commander Young's definition was implicitly the one adopted by Mr Tyrrell in 1935 in the pointer-apparatus experiments when six persons out of 29 (apart from the undoubted psychic, Miss Johnson) obtained pseudo-significant results. When the flaw in what may be called for brevity the

Young Theory of Random Sampling was demonstrated, 'the results', Mr Tyrrell reported, 'astonished us all.'

The crux of Wing Commander Young's opinion is in paragraph five where (the word 'random' having been dropped) it emerges that: 'In the last analysis, "haphazard" can mean no more than lacking in any *apparent* order . . .' But the question arises: '*Apparent* to whom? What may be 'apparent' randomness to one person may not be 'apparent' to another. For this reason statisticians have resorted to specific *tests* of randomness—such as those outlined in my paper—to determine whether or not there is order, apparent or unapparent.

Consider the following unhappy example. To 33 persons who took part in the Tyrrell pointer experiments, either as percipients, experimenters, or both, *no order was apparent*. But to the penetrating eye of Mr Fisk the case was otherwise. He demonstrated that Mr Tyrrell's conception of randomness—and therefore Wing Commander Young's now—was in need of amendment. He showed, not by one method, as is sometimes supposed, but by a variety of them—Fisk Flexible, Fisk Rigid, and Fisk Ordinary—that not only the percipient but the experimenter also could create astronomically significant results. He further demonstrated the method successfully in a certain type of ESP card experiment.

It may be objected that experiments of the pointer type are not done nowadays, and so such an error will not be repeated. So far as it goes, this may be true, I hope. But all history shows that our errors are known only after they are committed. Going back a long way it is clear from the tables that the Sidgwick and Smith targets were nonrandom to a prodigious degree; more recently, the Brugmans-Van Dam experiments appear to fall into the same category; the Turner-Ownbey and the Riess targets can be shown to be nonrandom, though far less so than in the preceding cases. Other examples can be cited. And what is one to say of the asserted 'displacement effects' in the Home-Testing ESP Experiments (*Journal S.P.R.*, 37, 1953, 14), which Dr Soal and Mr Bateman describe as 'A partial confirmation of certain Shackleton effects?' I suggest that a more likely explanation may be the loose experimental conditions in conjunction with nonrandom targets.

Let us put out of present consideration the three or four cases on record of consistent high-scoring under strict experimental conditions, and turn to the two or three hundred other cases on record. I hold no brief for Mr Spencer Brown, but it is impossible not to observe the sort of challenge he is asked to meet. Always it is in terms of two or three famous cases. No one challenges him to explain the Bastin and Green affair, the Home-

Testing experiments, and a great variety of others. It would be sanguine on Mr Spencer Brown's part to suppose that all such experimental results can be explained in terms of nonrandomness, but it would be rash on the part of his critics to hope that nonrandomness is not relevant.

In June 1937 Dr Soal in a paper in the *Journal* drew attention to a curious effect shown by the Tyrrell mechanical selector. It appeared that the pairs of digits (1, 1) (1, 2) . . . (5, 5) as they appeared in the target sequence, departed more extremely from randomness (i.e., equality) than would be expected by chance. Before re-reading the Soal paper I had found exactly the same nonrandom patterning in the Riess and Turner-Ownbey experiments. In the Tyrrell case the effect may not have mattered, since the digits still had to pass through a commutator. It is doubtless immaterial in the Riess case, but it may provide a new source for mirth in the immortal comedy of Miss Turner and Miss Ownbey. For certain types of recent experiments this form of nonrandomness may prove very damaging.

I think the moral is this. Nonrandomness always catches the unfortunate experimenter unawares. It never strikes in the same way twice. Tyrrell was caught off guard; Soal and Bateman were taken unawares with the ABA patterns—so nonrandom that the researchers' investigation was blocked at various places. Different from either of these is the Oram case. I have already described the invalidated dowsing experiment, and also the affair of the three subjects who received nonrandom targets, with the result (in the experimenters' opinion) of masking a genuine psychical effect, which may be described as Spencer Brown in reverse. This last may become increasingly important in PNC work, and experimenters would be rendering a service to the future development of research if, on finding consistently *insignificant* results in their work, they examined the possibility that some peculiarity in the targets may be concealing some genuine PNC effect. The problem is a decidedly ticklish one, but methods might be developed for remedying it.

Wing Commander Young finds it a 'preposterous notion . . . that haphazard [*sic*] arrangement is an "inescapable requirement" of ESP experiments'. I can make no claim to originality for the 'notion', since it is to be found in any serious statistical textbook, stated in plain English or implied. It is a fundamental prerequisite that must be met if tests of significance are to be applied. The point is that if statistical procedures are used on nonrandom data the results may be *spurious*—we may be deceived into thinking significant effects are there when in fact there are none—or we



may be fooled into thinking we have found no effects when in fact there are such.

J. FRASER NICOL

Cambridge,  
Massachusetts.

#### AN EXPERIMENT WITH RANDOM NUMBERS

SIR,—In his paper 'An Experiment with Random Numbers' (4) Mr Oram reports some results obtained with the 'Random Sampling Numbers' prepared by Professor M. G. Kendall and myself and published as No. 24 in the series 'Tracts for Computers' (3). As one of the authors, may I make two points which your readers would do well to consider in evaluating Oram's work.

First, the 100,000 numbers of Tract No. 24 were run off by myself and were then subjected by Kendall to several tests for randomness before publication. The procedure is set out in (1) and (2). One of the tests was known by us as the Serial test and was based on the frequency distribution of the 100 possible pairs of digits as they occurred when the numbers were read from left to right across the rows (as one reads the text of a book). The frequencies are to be seen in Table I on p. 55 of paper (3).

Kendall applied the serial test to all pairs of successive numbers and so, by taking the last number in the tables with the first, obtained 100,000 pairs. Each digit thus appeared twice, as the second and then as the first digit in a pair. The diagonal cells in this table record the occurrences of the pairs 00, 11, 22, etc. Oram to obtain his series A took pairs of columns in the published tables and noted the number of instances where like digits occurred in the same row. If Kendall had used each digit only once, he would have obtained 50,000 pairs, which would have been the 50,000 considered by Oram, and moreover the sum of the entries in the diagonal cells of the smaller table so obtained would have been the same as the number of 'successes' found by Oram. Oram's test for Series A is therefore a part of a test which had already been applied and was known to give an acceptable *P* value.

This comment does not apply to his Series B, which breaks fresh ground.

My second point is concerned with a question of principle. Oram says that 'now and then we need, particularly in view of certain criticisms, to have a simple factual reminder that our statistical methods, when tried out in the absence of any possible influence of psi phenomena, do give reliable results.' What has been overlooked in this paper is that the random sampling numbers

of Tract No. 24 were produced for a purpose and were carefully tested for local randomness before publication. As the author responsible for running off the numbers I can say (what does not detract from the value of the numbers as random sampling numbers) that I knew what they were for and what tests were to be applied; furthermore, I may say that having a measure of control over the operation of the machine I made use of that control to help to achieve my object.

Since the authors' purpose was that the numbers should have certain properties, which were derived in principle from a mathematical theory of probability, it is no test or confirmation of that theory that those properties should be found. From this point of view it is difficult to see how the results which Oram reports can be regarded as the 'simple factual reminders' of reliability which he requires.

On the other hand if Oram's tests agree, but not *too* well, with others previously applied, they afford a wider basis for saying that the work of constructing the tables was well done.

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B. BABINGTON SMITH

Yelford, Oxon.

5. 3. 55

SIR,—I am most grateful to Mr Babington Smith for letting me see his letter. I am sorry if it may have seemed unfair, either to him or to anyone else, that in my report I did not deal with his two papers, but I was anxious to keep the treatment as brief and non-technical as possible.

It seems that it is not true to say that my Series A is 'part of a test which had already been applied . . .'. The total score of 5,029 is that of a 50% sample of a set of data previously tested; but surely we can only assume that a 50% sample is not going to play tricks on us if we use the very 'laws of probability' that Spencer Brown has called in question? According to the quotation with which I opened my report, something odd was to be expected, in

spite of the tests applied by the authors of the table. It is, after all, not the only table that has been tested, although it may be the only one for which the tests have been so thorough throughout its entire length.

My cumulative-*t* test for Series A again refers to the 50% sample, but the most similar of the tests carried out by the authors seems not to have covered specifically, even at the 100% level, just this class of 'score'.

I have referred again to Babington Smith's references (1) and (2) and it seems to be perfectly clear that my frequency table, for the frequencies of column scores in Series A, is in no way 'part of a test which had already been applied'. There appears to be no evidence in either of his papers to suggest that the coincidences were considered with reference to the column totals. The frequencies within the table seem to be as independent of his results as they are of my own total score of 5,029, and he would not question their validity because of their necessary link with this aggregate value.

Babington Smith's second point is also not quite fair; it rather implies that his particular set of random sampling numbers is to be considered apart from all others. Spencer Brown had attacked random numbers in general, but he has also mentioned this set and in particular the asterisked blocks. Other published tables have been tested, and they too were produced for a purpose—much the same purpose that Babington Smith had in mind. They too may have been adjusted in the making, which was one of the points of Spencer Brown's criticism when he addressed the Society. I set out to conduct a test of the claim that I quoted, in the light of the general background of Spencer Brown's criticisms.

Babington Smith explains that his tables were arranged so as to have certain properties derived from the theory of probability, and he says that it is no test or confirmation of that theory that those properties should be found. But surely if some new tests are applied and found to give results that are in keeping with those that were obtained before, to the extent that the theory of probability predicts that this will happen some modest 'test or confirmation' has been effected? And in spite of what seem to me to be his mistaken criticisms of my Series A, Babington Smith does admit that the whole of my Series B was entirely new.

A. T. ORAM

Purley, Surrey.

SIR,—The most important QD in Mr Oram's experiment (*Journal*, 37, 369–77) was 169 in a series of 48,000 digit-pairs, or



say 0.35 per cent. Mr Oram seems to offer as a possible explanation the suggestion that he was unconsciously influenced (with the help of psi, presumably) to use an experimental design which would be favourable to the thesis of Mr Spencer Brown; while Mr Fraser Nicol mentions as a possibility that the numbers used by Mr Oram might in fact be in some sense random, despite their failure to pass what appears to be a perfectly good test of randomness.

Isn't all this a little far-fetched? A much more down-to-earth explanation lies to hand, namely that during the construction of the Kendall-Smith tables a small proportion of the digits got changed in the process of transcription—either from randomising machine to manuscript, or from manuscript to printed page. This hypothesis can be elaborated in detail and made quite plausible. Until it has been examined and disposed of, it seems premature to search for more profound explanations.

Meanwhile the much more important question remains: if published random numbers are defective, what can we do about it? I would like to offer a suggestion which is I think<sup>s</sup> new and may prove practicable.

Suppose we have an imperfect randomising device, turning out digits with a certain degree (not necessarily constant) of bias. Then it can quite easily be shown that, by adding together the first  $N$  of these digits and writing down the final digit of the resulting sum, repeating this process for the next  $N$ , and so on, we get a new sequence of digits which must be less biased than the original one. Moreover, if we can set any limit to the degree of bias of the randomiser, a formula states, in terms of this limit and of  $N$ , what is the maximum bias possible in the derived sequence, and this decreases towards zero as  $N$  increases.

The most convenient form of limit to adopt is *the minimum probability of appearance of any digit*. Let this be  $p'$  in the original sequence produced by the randomiser. Then the formula shows that it is

$$\frac{1}{10} - \frac{1}{10}(1 - 10p')^N$$

in the derived sequence.

Thus suppose, for example, we start by affirming: 'this randomiser may be very biased, but I am sufficiently convinced that it is never so biased that one of the digits has a chance of appearing which is less than one fifth of what it should be (i.e. 0.02 instead of 0.1).' Then  $p' = 0.02$ , and we find that, if we add together the output of the randomiser in groups of 10 digits and write down the last digit of each sum so obtained, the chance of appearance of any digit in the new sequence can never at any time be less than 0.09. If we take groups of 20 the figure rises to 0.099. This

degree of residual error should be easily outshone by any respectable psi effect.

In applying this method in any particular psi experiment, the aim should be to reduce the bias to a point substantially below the level at which psi is expected to manifest itself, and to choose the original assumption about bias in the randomiser so pessimistically that no reasonable person would prefer, to the hypothesis of psi, the hypothesis that the randomiser was in fact even more defective than this.

For the technical reader I should add that the above short account has been simplified so far as to contain minor inaccuracies and probably obscurities. Also the formula can be usefully generalized. I would be glad to communicate further details to anyone interested.

One important limitation should be mentioned here : the result quoted assumes complete independence of separate terms put out by the randomiser. It seems likely that interdependence is in practice a more serious defect of random sequences than simple bias (this would appear to be the case with the Kendall-Smith tables). Unfortunately such interdependence might take many forms and is not so easily limited in terms of a single parameter as is simple bias. The only obvious extension of the ' $p$ ' used above turns out to be insufficient. Further research is needed to find a reasonably plausible means of describing a limit to interdependence which will respond favourably to the above method of condensation.

It should be emphasised, however, that the method described allows any degree of fluctuation in the bias, within the limits set by  $p'$ , however wide or irregular, provided such variation is not dependent on what has gone before.

CHRISTOPHER SCOTT

Paris.

### ESP EXPERIMENTS WITH MARIA

SIR,—In view of the publication of an unsigned note in the *Journal*<sup>1</sup> with regard to Maria, the Spanish card-sensitive who has been investigated by my wife and myself, a brief explanation seems called for from her investigators.

We cannot at the present time publish a scientific paper on our researches because in December the Ciba Foundation will be publishing the paper read by one of us to the Foundation's recent

<sup>1</sup> 'News and Notes,' June 1955.

Conference on ESP. After that publication a full report will be available. In the meanwhile the following are the bare facts :

In November last my wife, acting on a sudden whim, resolved to test three Spanish servants with a pack of Zener cards. The first run of each girl in turn produced 16, 16, and 15 correct guesses.

My wife informed me and Dr E. J. Dingwall, who was staying with us, and we at once repaired to the kitchen. We there observed the three girls consistently guessing an average of over 11 correct guesses per run of 25 cards. The conditions were in no sense rigorous as we neither had the resources for creating laboratory conditions nor were we expecting the very sensational results. However, the three observers were fully conversant with the main types of sensory leakage to be expected and were unable to detect any such leakage.

The final figures with the three girls acting as agent and percipient in turn were as follows :

<i>Runs</i>	<i>Correct guesses</i>	<i>Expected</i>	<i>Average</i>	<i>C.R.</i>
76	855	380	11.2	27

Domestic considerations prevented any further testing (to date) of two of the girls, but Maria fortunately remained with us.

In the course of the following weeks more satisfactory experimental techniques were instituted. The cards were enclosed in opaque covers and manipulated in a manner which ruled out any information reaching the percipient from back or face. There still remained the possibility that sensory information was being carried from agent to percipient by glances at the correct key card since no screen was being used. It is therefore of the utmost importance to observe that during one fairly long series 'clairvoyant' and 'telepathic' methods were alternated.

The 'clairvoyant' method was to place the pack, unseen after shuffling by the agent, on the table between the agent and Maria and to have her match card by card (each in its opaque cover) with the key cards.

In this series the 'clairvoyant' runs actually produced *more* correct guesses than the telepathic. This result suggests that whatever the explanation it could not be sensory leakage from the agent glancing at the correct key cards. The average correct guesses per run in this series of 'clairvoyant' results was 10.0 and for the corresponding 'telepathy' runs 9.9.

The total results for all runs with cards covered *and ignoring the first spectacular results with bare cards* were :

<i>Runs</i>	<i>Correct guesses</i>	<i>Expected</i>	<i>C.R.</i>
810	5848	4050	32 (approx.)



Towards the end of the series, Maria showed signs of boredom, and domestic circumstances were such as might be expected to upset the experimental *gestalt*. However, her final scores began to rise after a temporary fall to chance level and, judging by previous results, would have continued to rise had the experiments been continued. We were interrupted by preparations for a two months' visit to England.

On our return a new experiment was begun. Maria remained at chance level for some time but gradually worked up a C.R. of 4.1. The last stages of this experiment were observed by Dr Soal and Mr Bateman who through the good will of Mrs Eileen Garrett were able to come out and carry through a brief confirmatory investigation.

They at once pointed out, what we would never have disputed, that the experiments without a screen laid themselves open to the criticism that sensory leakage might take place from the agent, but after observing a number of runs they agreed that they were unable to find any evidence that this was so. They put this to the test by carrying out an experiment with Mr Bateman as agent concealed from Maria by a screen. Once more Maria began at chance level and it was not unlikely that she was overawed by the attentions of two important gentlemen from abroad. (She is a 16-year-old housemaid.) Eventually her guesses began to improve in a way which was astonishingly similar to that in which they had improved in the previous experiment to test which this one had been designed.

After comparing the two results, Dr Soal and Mr Bateman agreed that the type of sensory leakage ruled out by the Bateman experiment was very unlikely to be the explanation of the similar result in the other experiment. Thus this evidence, added to the evidence afforded by the earlier 'clairvoyant' results, suggest that the whole series of results of testing Maria are worthy of the very closest attention.

While very properly not endorsing any part of the experiments which he had not seen himself, Dr Soal summed up his impressions of Maria in words which he wrote in our copy of Soal and Bateman: 'During the past ten days I have witnessed a statistical demonstration of the powers of the third card-guessing sensitive that it has been my privilege to meet.'

This makes Maria important enough perhaps, but we cannot help regarding the original testing of the three servant girls, with all its obvious technical shortcomings, as being even more important, and we are as prepared to accept the necessity of invoking ESP to explain this unique result as we are to

invoke it for the experiment with Maria supervised by Dr Soal.

Directly domestic circumstances make it possible, a long series of further experiments will be begun. Their objective will not be to 'prove' ESP once more, but to examine the *modus operandi*. It is hoped that more than one card-sensitive will be unearthed besides Maria herself, and we shall be glad to carry out any experiment for those whose study of ESP is complicated by lack of human guinea-pigs. Any further experimentation will be carried out in accordance with the requirements of rigorous laboratory technique. We shall be particularly glad of suggestions for new forms of experiment.

Our thanks are due to the Ciba Foundation for permitting us to publish this preliminary report in the *Journal* and to Mrs Eileen Garrett, Dr S. G. Soal, Mr F. Bateman and Dr E. J. Dingwall for their interest in our work.

JOHN LANGDON-DAVIES

PATRICIA LANGDON-DAVIES

San Feliu de Guixols,  
Gerona, Spain.

### POLTERGEISTS: A PHYSICAL THEORY

SIR,—May we be permitted space for a brief reply to Mr G. W. Lambert's article on 'Poltergeists' in the June *Journal*?

The theory therein advanced—that the phenomena may be accounted for by underground water and analogous physical causes—can surely only be a possible solution in a small percentage of the cases hitherto recorded. Some such causal factors might perhaps be held to account for strange sounds (such as creakings, rappings, etc.) or even for the occasional movements of furniture; but what of those cases in which fires were lighted, small objects thrown across the room, stones falling from the ceiling, inexplicable touches, windows broken, dishes whisked off the table, etc.? Many of these phenomena were observed on the second story of the house, which was far removed from any water. Many of them were observed by sceptical and competent witnesses, over a period of days or weeks.

Mr Lambert, in speaking of our collection of cases, says: '... many of the cases as listed by them are lacking in detail, especially as to the position of the place affected. In making my list I have chosen cases which can be dated, anyhow as to the year of occurrence, and assigned to a definite small geographical area.'

In reply may we point out:

(1) That every case referred to in our book *was* dated—the year being given.

(2) That the case was necessarily summarized in a few lines, but that references were given in every instance to the original, detailed accounts.

(3) That it seems unfair to us to limit the discussion to 'a definite small geographical area', instead of taking into consideration *all* the cases on record.

(4) That it seems strange to us that 'the stories have nearly all been chosen from F. Podmore's paper in *Proceedings*, Vol. 12', which appeared in 1896, and to stress Andrew Lang's article in the *Encyclopaedia Britannica* (1911), when much newer evidence is surely available.

(5) That the reference to Father Thurston's conclusion that he was 'unable to dismiss it as all delusion and deceit, but cannot believe that the effects alleged were caused by a tricky *spirit*' is surely beside the point, when no competent psychical researcher has maintained this.

(6) That Mr Lambert's list of 'References' seems to us most incomplete, since it neglects to mention Harry Price's *Poltergeist Over England*, Dr Fodor's psychoanalytic studies of the poltergeist, our own *Story of the Poltergeist down the Centuries*, *The Great Amherst Mystery*, *The Bell Witch*, and others too numerous to mention.

In view of the above, may we not feel justified in concluding with the following quotation: 'The last word has by no means been said concerning poltergeists, and, should the reader feel stimulated to consult the original accounts, he will find it well worth his while to do so.'

Hollywood, California.  
New York, N.Y.

HEREWARD CARRINGTON  
NANDOR FODOR

SIR,—I do not think that the physical theory for alleged poltergeist phenomena (*Journal*, 38, 49-71) propounded by Mr G. W. Lambert is a valid one. I suggest that his theory does not explain many of the more incongruous features associated with such phenomena.

Disturbance of the earth's surface, of such violence as to cause the movement of physical objects over a distance of many yards (not uncommon in alleged poltergeist phenomena) would not be dissimilar from that of a minor earthquake and, for that reason, might be expected to result in visible and permanent structural damage (e.g. cracks in walls and ceilings) to buildings. I do not know of any building, in which such phenomena are alleged to



have taken place, that has sustained such damage. Window glass is likely to sustain direct damage in a way that walls and ceilings are not.

Alleged poltergeist phenomena are associated with both 'movement' and 'displacement' of physical objects :

(a) The hands of a clock (occupying its original position) may be said to have been altered, or the pendulum of a clock (occupying its original position) may be found to have been twisted (displacement of physical objects where there are no similar objects in close proximity thereto).

(b) The hands and/or the pendulum of a clock (occupying its original position) may be said to have been altered, or be found to have been twisted whereas the hands and/or the pendulum of an identical clock (occupying its original position) have not (displacement of physical objects where there are similar objects in close proximity thereto).

(c) Two identical pieces of china may occupy a similar position (e.g. both on the mantelpiece) and stand on a similar surface. One of such pieces may be found to have changed its position whereas the other has not (movement of a physical object where there is a similar object in close proximity thereto).

(d) Physical objects may be found in positions, relative to one another, which suggest that human agency was responsible for that fact (movement and displacement of physical objects which may or may not have been in close proximity to one another).

(e) Physical objects may be found many yards from their alleged former position in circumstances which exclude any causal relationship between them and disturbance of the earth's surface. Such objects may be found in a closed drawer ; in the garden ; or in another room with the only communicating door closed and intact (movement with or without displacement of physical objects).

I suggest that those phenomena contained in paragraphs (a), (b), (c), (d) and (e) and why one physical object should be affected whereas the other is not, with our present knowledge, are inexplicable.

I agree with Mr Lambert that because a phenomenon is tentatively inexplicable—tentatively is the operative word—that fact does not commit one to accepting an 'explanation' that is paranormal. It is dogmatic to assert—where little data is available—that a phenomenon, which we do not understand, because we do not understand it and which does not conform with hitherto recognised hypotheses, does not conform with natural (or normal)

law. I suggest that a rational explanation should be sought where that is possible and that the onus of proof, in phenomena of a recognised and established nature, is upon him who alleges a departure from the norm, i.e. upon him who denies their recurring validity. In phenomena, largely unamenable to empirical or laboratory investigation and analysis, inductive processes of reasoning may alone be available.

DENIS CHESTERS

Brighton, Sussex.

Mr Lambert's comments on the above are as follows :

With reference to the letter from Mr H. Carrington and Dr N. Fodor, I hasten to make it clear that nothing in my paper was intended as a criticism of their collection of cases in *The Story of the Poltergeist down the Centuries*, to which I made reference on p. 50 of my paper. The treatment of each case in that collection was necessarily brief, but for my purpose it was important to have as much detail as possible. For that reason I chose for my list cases from collections in which the evidence was set out at sufficient length for an interested reader to check most of my statements about points of detail. For my purpose it was also necessary to know not only the year of the occurrence (better still, the month and days of the month), but also the *location* of each case, at any rate within a mile or two, so as to be able to correlate meteorological and geological data. It is not enough to show that a particular case occurred (say) in the year 1771, and that that was a very wet year. It is also necessary to show that it occurred at a place where local conditions made subterranean flooding very likely to occur. As I pointed out on p. 50, the exact location of a case is sometimes purposely concealed, and it is no reflection on subsequent historians of such cases if they cannot accurately locate them. My reference to a 'definite small geographical area' related to the place of each case, and did not limit the scope of the discussion in any way.

My list of 54 cases, of which the dates and places are known, could no doubt have been extended by further research. It was not meant to be a complete catalogue of cases, like that in *The Story of the Poltergeist down the Centuries*, but to be a fair sample, and such I still believe it to be. The examination of those 54 cases in the light of meteorological, geological and, in some cases, tidal phenomena was a heavy task, requiring many hours of work in reference libraries, and many fruitless searches. It took all the time I could spare. I invited my readers to carry further the testing of the working hypothesis (p. 62), but no critic has yet cited a

single non-fraudulent case, the primary effects of which cannot be explained along purely physical lines. All the occurrences mentioned in the second paragraph of the letter can be accounted for by the sudden tilting of the house affected. As the second storey, in such an event, moves through a longer arc than the ground floor, occurrences due to tilting are more likely to happen upstairs than downstairs. As regards fires, these were usually caused in earlier times by the ejection of live coals from fireplaces. Nowadays the sudden breaking of live electric circuits by the movement of the house is liable to cause fires in various places. The attribution of such fires to arson was, in my view, usually an error of inference, like that which led to the blame for the movement of objects being attached to adolescents (p. 61).

Turning now to the numbered points in the letter :

(1), (2) and (3) have been answered above.

(4) Of my four main sources, the last two, Sitwell's *Poltergeists*, and Thurston's *Ghosts and Poltergeists*, were published in 1940 and 1953 respectively. My own view (not widely shared, I admit) is that recent cases are not necessarily better evidenced than old ones.

(5) The words quoted (from the first paragraph of my paper) do not represent the view of Father Thurston, but the presumed view of Andrew Lang, as shared by many people today.

(6) The short list of references in Appendix II is not a bibliography, but merely an explanation of the abbreviations used in the paper. As such, it is complete. Further, if in a given case the primary effects are due to the kinds of cause I have assumed, obviously psychoanalysis cannot help. But it may throw important light on some kinds of secondary effects, with which I did not purport to deal (see p. 61).

I am glad to find myself in complete agreement with the quotation at the end of the letter.

Mr Denis Chesters, in the second paragraph of his letter, remarks that he does not know of any building in which poltergeist phenomena are alleged to have taken place, that has sustained permanent structural damage in the way of cracks in walls, etc. Such damage would be caused not by mere noises, but only by physical lifting, lowering, or shaking of the building, and that to a sufficiently severe extent to give rise to permanent damage. In the south of England such damage is seldom found, but in the industrial north, where there are considerable areas in which subsidence due to mining is prevalent, such visible signs of settlement are to be found. Mr Trevor Hall, F.R.I.C.S., a member of the Society who is Perrott Student of Psychical Research, and is by profession a Surveyor, has sent me particulars of two buildings



in Yorkshire of which he has personal knowledge. One is a Rectory, 'haunted' by a poltergeist, the stonework and window heads of which show typical signs of subsidence; the other an old house, with a long reputation of being haunted, which is now uninhabitable owing to the same cause. He also mentions the case of a Church in Essex, reputed to be haunted, which has a bad crack in the tower, and is now condemned as unsafe. The church is on an estuary, near the sea.

Mr Chesters next cites, in general terms, five kinds of cases in which material objects may be found to have been moved in ways that show selection, implying human activity as the cause of the movements. Undoubtedly such cases occur, but as the movements are found to have taken place and are not seen actually occurring, it may be and often is a false inference that the movement took place without ordinary human intervention. I mentioned an obvious case on p. 54 as having occurred in the Château de T. The fact, for instance, that an article is found on the floor of room B, which was last seen by the householder in room A, does not prove that it passed (say) through two intervening closed doors and round a corner in the passage. Such suggestions would not be made if there had not previously been unaccountable movements to mystify the occupants of the house. One would like to learn of a case in which one of these extraordinary kinds of movement was the *first* and only movement noticed.

Another correspondent suggests that I have fallen into the error of accepting only that evidence which fits in with my theory, and of twisting the facts to fit the theory, because the theory will not fit in with all the known facts. No instances are quoted to substantiate this accusation, and in the circumstances I can only point out that I have not ignored the 'occurrences' which cannot be attributed to ordinary physical causes, but have assumed that the accounts of them were due to secondary elaboration or to malobservation. In the final paragraph of my paper I expressly disavowed any claim to have explained by the theory *all* cases of alleged poltergeist phenomena. Mixed cases, in which two or more quite different kinds of cause are at work, are extremely difficult to unravel. Nor does the theory explain the phenomena of mediums who claim to be such. In cases of the kind dealt with in my paper, the reputation of being 'an unconscious medium' is fastened by others to the unfortunate individual concerned, with the result, in some cases, that that person resorts to fraud to maintain the dangerous popularity he or she has gained. When that has occurred, movements of objects which can only be attributed to human agency of course occur.

So far, I have received no criticism in detail of the hydraulic theory as such. Most correspondents claim that the theory does not cover all 'poltergeist' cases, a view with which I agree.

#### DR GELEY'S REPORTS ON THE MEDIUM EVA C.

SIR,—If indeed Dr Thouless's language is intended to cast doubt on Herr Lambert's veracity, I am bound to put on record the fact that Dr Osty showed me the same photographs and made the same inference from them. He insisted on secrecy, which in the circumstances he was entitled to do, and I made no note. I am therefore unable to fix the date of this conversation, for at the time I saw Dr Osty frequently.

It should be added that Dr Osty must have conveyed his conclusions, perhaps without the evidence, to a good many people, for, as Mr Salter says, the matter was freely discussed, to my own knowledge, between about 1927 and 1935.

THEODORE BESTERMAN

Geneva.

### NEWS AND NOTES

#### *Cambridge Conference on Spontaneous Phenomena*

This Conference, organized by the Society (see 'News and Notes' in the June issue), took place at Newnham College from 11-16 July. Its members, including observers and those who attended for part of the time, were : Mrs Lydia Allison (U.S.A.), Robert Amadou (France), Professor C. D. Broad (U.K.), A. D. Cornell (Conference Information Officer), Mrs Laura Dale (U.S.A.), Professor C. J. Ducasse (U.S.A.), Martin Ebon (Administrative Secretary, Parapsychology Foundation), Mrs K. M. Goldney (U.K.), Professor Hornell Hart (U.S.A.), Mrs Rosalind Heywood (U.K.), Miss Ina Jephson (U.K.), G. W. Lambert (U.K.), Dr Margenau (U.S.A.), Dr L. Mars (Haiti), F. Masse (France), Dr C. A. Meier (Switzerland), Dr Gardner Murphy (U.S.A.), Professor H. H. Price (U.K.), Dr J. B. Rhine (U.S.A.), Dr Louisa Rhine (U.S.A.), W. G. Roll (U.K.), D. C. Russell (U.K.), W. H. Salter (U.K.), Mrs H. de G. Salter (U.K.), Dr E. Servadio (Italy), A. Slomann (Denmark), Professor F. J. M. Stratton (U.K.), Professor W. H. C. Tenhaeff (Netherlands), Dr R. H. Thouless (U.K.), Dr Gerda Walther (Germany), Dr Th. Wereide (Norway), Dr D. J. West (U.K.), G. Zorab (Netherlands).

The following papers were read and discussed : 'What Contribution to Psychical Research can be made through the Investigation

of Spontaneous Cases?' (Dr Gardner Murphy); 'Phantasms of the Living and the Dead: the traditional method of research' (W. H. Salter); 'The Experimental Approach with special reference to Travelling Clairvoyance' (Professor Hornell Hart); 'Haunts and other Localised and Iterative Phenomena' (Professor F. J. M. Stratton); 'The Psychology of Spontaneous Cases' (Dr C. A. Meier and Dr E. Servadio); 'Poltergeists: history and methods of investigation' (G. W. Lambert and Mrs K. M. Goldney); 'Recent research work on Spontaneous Cases at the Parapsychology Laboratory, Duke University' (Dr J. B. Rhine and Dr Louisa Rhine).

The Conference was made possible by generous financial support from the Parapsychology Foundation, New York. In expressing our gratitude to the Foundation, we also extend our sympathy to its President, Mrs Eileen Garrett, who was prevented by illness from attending it.

### *Myers Memorial Lecture*

As announced in the *Journal* for March, the twelfth Frederic W. H. Myers Memorial Lecture will be given by M. Gabriel Marcel. M. Marcel's lecture, which will be in English, will be entitled 'My First Encounters with Parapsychological Facts and their bearing on Philosophy'. The lecture will take place at 6.30 p.m. on Wednesday, 26 October at Caxton Hall (Great Hall), Caxton Street, Westminster, S.W.1.

### *Presidential Address*

The subject of Mr G. W. Lambert's Presidential Address, which will be delivered at 6.30 p.m. on Thursday, 1 December at Caxton Hall (York Hall), will be 'The Use of Evidence in Psychical Research'.

### *Ichthyosis and Hypnosis*

Great interest, in view of its bearing on the problem of mind-body relationship, was aroused by the publication in the *British Medical Journal* of 23 August 1952 of a report by Dr A. A. Mason of a case of congenital ichthyosiform erythrodermia of Brocq ('Crocodile Skin') which he treated by hypnosis. The report was summarised in the *S.P.R. Journal* for November 1952, and an abstract of subsequent correspondence in the *British Medical Journal* was published in our issue for January 1953. It will be recalled that the condition, which had previously been regarded as resistant to all forms of treatment, showed great improvement and that this took place limb by limb in direct response to the sugges-



tions given under hypnosis, thus ruling out the possibility of spontaneous resolution. In a letter published in the *British Medical Journal* for 2 July 1955 Dr Mason records the patient's condition in January of the present year, when he re-presented the patient, then nearly 19 years of age, to the Dermatological Section of the Royal Society of Medicine.

Not only [writes Dr Mason] has there been no relapse, but his skin has continued to improve. What is also surprising is that this improvement should continue without further treatment of any sort, hypnotic or otherwise. The patient is in no doubt as to his improvement, and comparison of photographs confirms this. There is still considerable involvement of feet and some involvement of legs and thighs, but all much less severe. The arms and hands are quite clear, and abdomen, buttocks, and back are only slightly involved. Nowhere is there evidence of cracking and infection, which had been such a disabling and painful complication previously. The smell has also quite gone.

I attempted to hypnotize the patient again, and it is curious to relate that, while on discharge he had been a good hypnotic subject, he was now quite unhypnotizable. I tried again two weeks later and this time succeeded, but further suggestions made in the hypnotic state that his legs would improve did not prove efficacious.

Dr Mason's letter concludes with a reference to a case of pachyonychia congenita (Mullins, J. F., *et al.*, *Arch. Derm. Syph.* (Chicago), 1955, 71, 265), a congenital condition closely allied to Brocq's ichthyosis, which also responded favourably to hypnotic suggestion, showing that 'the treatment is repeatable in other hands and that the improvement in the case I reported is not a freak occurrence'.

### *What Happened at Versailles?*

Many criticisms have been made of the evidence presented by Miss C. A. E. Moberly and Miss E. F. Jourdain in *An Adventure*, but the good faith of these ladies has never, to our knowledge, been called into question. Considerable surprise was therefore caused when the *Spectator*, on 24 June, printed under the above heading a letter from Mr Walter M. Wigfield, Headmaster of the County Grammar School, Godalming, Surrey. Referring to a review in the previous issue by Professor Antony Flew of the fifth edition of the book (see Mr W. H. Salter's notice in our June number), Mr Wigfield said that he was 'amazed to find this ancient and temporarily successful hoax taken seriously at this date'. He had not cut out the obituary notice, but he had 'the clearest recollection' of Miss Moberly's confession during her last illness that the whole story was a hoax.

Following reasoned protests in the next issue from Dr Joan Evans, Miss Moberly's Literary Executor, and Mr J. R. Sturge-Whiting, who had severely criticised the story on evidential grounds in his book *The Mystery of Versailles* (1937), Mr Wigfield wrote in the following issue that the article to which he had originally referred was not an obituary notice but 'an article published much more recently'. This has not yet come to light, but even if Mr Wigfield's memory of its existence proves more reliable than his recollection of the obituary notice, it is hardly likely to impress those who have made a close study of the case.

#### *Report on Borley Rectory*

At the Council's invitation, Dr E. J. Dingwall, Mrs K. M. Goldney, and Mr Trevor Hall have been at work on an appraisal of the evidence on which Borley Rectory has been claimed to be 'The Most Haunted House in England'. Their report is now in the press and will shortly be published in the Society's *Proceedings* under the title *The Haunting of Borley Rectory: a critical survey of the evidence*. A commercial edition will be issued simultaneously by Gerald Duckworth & Co. Ltd.

#### *The Investigation of Spontaneous Cases*

Advice to investigators of spontaneous cases is given in a pamphlet soon to be published by the Society under the title of *Notes for Investigators of Spontaneous Cases*. A copy will be sent free to every member who applies for it, and it will be available to the public at 1s. With *Hints on Sitting with Mediums* (6d.) and *Tests for Extrasensory Perception* (1s. 6d.), guidance on research in the three main departments of the Society's field of inquiry will be available in printed form.

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